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INSTINCT AND VALUE*

By Henry C. Link

"We may say, then, that directly or indirectly the instincts are the prime movers of all human activity. The instinctive impulses determine the ends of all activities and supply the driving power by which all mental activities are sustained; and all the complex intellectual apparatus of the most highly developed mind is but a means towards these ends, is but the instrument by which these impulses seek their satisfaction,—and in them we are confronted with the central mystery of life and mind and will."

W. McDougall, "Social Psychology," 44.

When, many years ago, Bohn asked: *"Qu'est-ce que l'instinct? Un mot;"* and Condillac remarked: *"L'instinct n'est rien;"* they expressed an opinion which finds little favor today. Instincts are now recognized as genuine forces and of supreme importance. Within recent years whole philosophies, social systems, economic structures have been erected with instincts as their foundation. The quotation given above represents a view in which instinct, far from being a mere word, is the prime factor in human activity; far from being nothing, it is responsible for everything.

When, more than half a century ago, the "marvels and mysteries of instinct"[1] first became subjects of considerable interest, the general opinion was that the instincts were God's peculiar gift to animals just as reason was His peculiar gift to man. "We may call the instincts of animals," say Kirby and Spence,[2] "those faculties implanted in them by the Creator, by

*Abstract of a Dissertation presented to the Faculty of the Graduate School of Yale University on the Candidacy for the Degree of Doctor of Philosophy in 1916.

which, independent of instruction, observation, or experience, and without knowledge of the end in view, they are all alike impelled to the performance of certain actions tending to the well-being of the individual and the preservation of the species;" but "man is, indeed, devoid of instinct; and his reason, if indeed it be of the same nature as that of the higher beasts, is as superior in its results as the instinct of the bee is to the instinctive turning of the plant to the light."[3] However, this distinction was soon overcome by a psychology which added to the old rationalistic method the method of objective observation which had already discovered the instincts in animals. Instinct was seen to be common to man as well as to animals, and it now became a question of what were the relative positions of instinct and reason in man.[4] The impetus of rationalism tended, for a time, to keep reason on its throne. However, the rise of Darwin and Spencer, and the swelling train of evolutionary doctrines which followed after them, tended more and more to minimize the importance of reason, until reason was finally reduced to the position of the handmaid of instinct. Whereas, in the past, men looked for a divine or rational principle to guide their conduct, they now turn to an analysis of their instincts. These, as James has so vividly shown, are not rational: they simply are,[5] and as such, must be taken at their face value. Therefore, the tendency today is to define character as a mosaic of instincts and emotions.[6] The finer sentiments and ideals of man are simply slight refinements of the instincts. The reversal is complete. Whereas the instincts were wont to be explained as the gift of God, God is now interpreted in terms of the instincts.[7] That is, the aims and values of the individual, as they are found in society, in art, and in the common pursuits of life, are simply the reflection of instinctive and emotional activities.

This almost Copernican change of emphasis marks a tremendous step in the direction of a clearer, more realistic conception of human nature. It opens up an entirely new realm for psychology and its various branches. It places the individual in a category where his behavior and the values he strives for

[1]G. Garratt: "Marvels and Mysteries of Instincts," 1857. Lord Henry Brougham: "Dialogues on Instinct," 1844. Jonathan Couch: "Illustrations of Instinct," 1847. T. L. Kemp: "Indications of Instinct," 1854.

[2]"Introduction to Entomology," 1858.

[3]T. L. Kemp, *op. cit.*, 138.

[4]Cf. H. R. Marshall: "Instinct and Reason."

[5]W. James: "Principles of Psychology," I, 386.

[6]Cf. Alexander Shand: "The Foundations of Character"; also W. McDougall: "Social Psychology."

[7]A notable example of this tendency is Benjamin Kidd's "Social Evolution." Almost all anthropological works adopt this view. Cf. Lippert's "Kulturgeschichte," introductory chapter.

may be made subjects of scientific research. It offers an opportunity for scientific speculation which has already greatly enriched the literature of the twentieth century.

Out of the valuable contribution to modern thought which the theory of instinct has made, however, there has risen also a serious danger. We are confronted today by a new kind of rationalism, the rationalism of instinct. This rationalism has substituted for the discarded innate ideas of an older, philosophical rationalism, a group of instincts, and with these as a starting point, has constructed systems which may some day seem just as fantastic and incoherent as the older rationalistic systems appear to us today. One of the most widely known examples of this type of rationalism is W. Trotter's exposition of the herd instinct. It will be remembered that Trotter attributes the entire range of social phenomena to four instincts, including the instinct of gregariousness. His particular hobby is the gregarious or herd instinct, to whose activities he ascribes a great range of human activities. The writer's interest in the problem of instinct was first aroused by the ease with which Trotter demonstrated the existence of the herd instinct and its responsibility for herd phenomena. To the writer it seemed almost as easy to demonstrate that all herd phenomena could be traced to the interlacing effect of instincts other than the herd instinct. In short, it seemed quite plausible to attribute all herd phenomena to other instincts *without reference to any herd instinct whatsoever.*

Probably the high water mark of psychological rationalism is McDougall's "Social Psychology." To the seven or eight primary instincts (including gregariousness) which he selects, McDougall attributes the fabulous wealth of modern social institutions. Strangely enough, McDougall himself condemns this kind of procedure on the part of philosophers. He quotes, in this connection, the assumption upon which V. Cousin bases his philosophy of history:

> "The various manifestations and phases of social life are all traced back to tendencies of human nature from which they spring, from five fundamental wants, each of which has corresponding to it a general idea. The idea of the useful gives rise to mathematical and physical science, industry and political economy; the idea of the just to civil society, the State, and jurisprudence; the idea of the beautiful to art; the idea of God to religion and worship; and the idea of truth in itself, in its highest degree and under its purest form, to philosophy. These ideas are argued to be simple and indecomposable, to coexist in every mind, to constitute the whole foundation of humanity, and to follow in the order mentioned."

McDougall then adds:

> "We have here the spectacle of a philosopher, who exerted a great influence on the thought of his own country, and who rightly conceived the relation of psychology to the social sciences, but who, in the absence of

[illegible] assumptions.[1]

McDougall's [illegible] the same thing. [illegible] method. [illegible] McDougall's instincts [illegible] concerned. [illegible]

There is no questioning the [illegible] mental and determining [illegible] But our knowledge [illegible] warrant any wholesale [illegible] tainty about the [illegible] rise. The writings of [illegible] ticular force the [illegible] claims made for instinct. [illegible] omists like Parker and Fisher [illegible] explanation of the values which [illegible] have hitherto taken for granted. [illegible] economic values in the light of instinct [illegible] but add to the richness of [illegible] At the same time, the need for a clearer definition of instinct and of the sphere within which we may attribute specific values to instincts becomes all the more imperative. Let us therefore [illegible] consider the definition of instinct in relation to the determination of values.

I. The Mechanistic Conception

A careful analysis of instinct leads [illegible] from a consideration of complex chain activities to more elementary reflex activities. The tropism as defined by Loeb is instinct in its simplest terms. The tropism is a definite physico-chemical mechanism within the organism which, when properly stimulated, will give rise to a specific response. According to Loeb, the organism is entirely composed of such tropisms or instinctive forces. "Our wishes and hopes, disappointments and sufferings, have their source in instincts which are comparable to the light instinct of the heliotropic animals."[2] Aside from the biological rationalizing by which Loeb has arrived at this comprehensive claim for his theory of instincts, there is another question which must here be considered. How can an organism made up of tropistic entities experience hope and suffering? What kind of tropisms are these? If the activities of the organism are simply

[1] Op. cit., [illegible]
[2] "The Mechanistic Conception of Life," 30.

a continuous series of tropisms, one mechanical unit impinging upon another, why should hope and suffering ever arise? There is implied here a conflict of instinctive forces, and a comparison of results which is quite foreign to the mechanical conception of instinct with which Loeb sets out.

As a matter of fact, Loeb quickly modifies his original idea of a tropism through the introduction of the concept "Unterschiedsempfindlichkeit," or 'differential response.' Instead, therefore, of simple instinctive forces, acting uniformly in the presence of a given object, we have tropistic forces that act largely in terms of past and present experience. The associative area, according to Loeb, is the center through which simple, mechanical activities are modified. With this qualification, the strictly mechanistic definition of instinct becomes so involved that it is impossible to select certain definite instinctive activities and attribute to them definite results and values. The tropism is now regarded not as an activity which determines the entire organism from time to time, but as an activity which is controlled or modified by the organism as a whole through the instrumentality of the associative area.

Loeb's mechanistic conception of instinct makes inevitable the conclusion that the organism as a whole enters into the determination of the part, and that the values sought by the instincts are dependent upon the values sought by the organism as a unit. And it is furthermore apparent, from Loeb's own statements, that the mechanical entities, the tropisms, which were intended to explain the actions of the organism, *do not even exist except in theory*. In reality, they are inseparable aspects of the organism and cannot be explained except in terms of the very organism which they were intended to explain. Obviously, the mechanistic concept as exemplified by Loeb has limitations as an explanation, whatever its value as a *method* may be. For the present, it is apparent that values cannot be explained or predicted, either for the individual or for society, upon the basis of mechanistic instincts. The mechanist may make any assumptions he pleases about life so long as those assumptions lead to fruitful results. But fruitful as have been the results of the mechanistic method thus far, they do not warrant the broad claims which psychologists have been making for instincts. These claims have grown rather out of a scientific romancing than out of careful and logical deductions from the facts which the mechanistic method has revealed.

II. The Behavioristic Method

The behavioristic definition of instinct is in general quite the opposite of the mechanistic definition. It usually insists upon the impossibility of explaining the character of an organism

as a collection of [illegible] other hand, that [illegible] characteristics which [illegible] mechanically [illegible]. Now [illegible] mechanistic [illegible] of mechanism [illegible] possible terms. [illegible] But when we ask the behaviorist [illegible] unique, [illegible] "What is behavior?", the answer is [illegible] is a function of [illegible] Instinct, according to this [illegible] a form of behavior.

Strange to say, the [illegible] of this definition of behavior destroys [illegible] the behaviorist insists upon, namely, the [illegible] contrasted with mechanical response. According to this [illegible] the response of a bell to a button is [illegible] as the response of a man when [illegible] process of release which is a function of [illegible] mechanism. In order to avoid this dilemma the behaviorist then advances all sorts of criteria for the [illegible] of an organism, but none of his criteria satisfies. To make a long story short, he is unable to define the organic except in terms of behavior, and at the same time he is unable to define behavior except in terms of an organism. What distinguishes an organism is behavior, but what distinguishes behavior is its organicity.

It becomes evident that behaviorism rests upon an assumption, more or less clear in the minds of its adherents, as to the types of action which shall constitute its field of research. It selects the organism, not by logical definition but by practical judgment. There is no logical difference between behaviorism as applied to organisms and behaviorism as applied in physics, chemistry, or biology. Every scientist is a behaviorist in his field. And absolutely no scientific reason is given why the behaviorist should consider his theory or the objects of his research unique, except the reason that *they are his*.[11]

In short, behaviorism as a theory or method rests ultimately not on a logical but on a practical judgment, a choice of values. As in the case of mechanism, behaviorism is not an explanation, not a philosophy. It is a method of investigation. And it is

[10] E. B. Holt: "Response and Cognition," *Jl. of P., Ps., & Sc. M.*, July 8, 1915, 371.

[11] The physiological explanation of behavior represented by H. L. Jennings only apparently escapes this statement. A criticism of physiological behavior will be made elsewhere.

for its usefulness in discovering and defining instincts that we must here value it. The essence of the behavioristic method is the establishing of certain relatively constant relations between certain stimuli and the responses of an organism or group of organisms. If it can be determined that a certain type of stimulus will almost invariably bring the same response from a certain group of organisms, then it may be assumed that the response is instinctive or habitual. The method is essentially a systematic attempt to correlate stimuli with responses.

The correlations thus far obtained in the field of human psychology through the behavioristic method, especially by Watson, tell us something about instinct. But it is certain that they do not tell us very much. On the basis of the behavioristic method, we have only the scantiest data upon which to base any conclusions regarding instincts. Certainly none of the results thus far justifies us in setting up anything but a tentative classification of instincts, far less in describing what certain instincts have accomplished, and least of all, what certain instincts will accomplish in the future.

It may be remarked that, although the behavioristic concept of an organism is antithetical to the mechanistic concept, the value of the behavioristic method is largely dependent upon the degree in which the mechanistic conception is true. Naturally, consistent correlations between stimuli and behavior cannot be formed unless there are those relatively independent tropistic entities which the mechanist describes. The difficulties which both the behavioristic and the mechanistic methods encounter in their search for such entities recur in another point of view from which the study of instinct is approached, namely, the part played by intelligence.

III. Instinct and Intelligence

Intelligence, no matter how defined, is a factor which greatly complicates the search for fundamental and relatively fixed instincts. Loeb identifies intelligence with the associative area, through which the tropistic actions are radically modified. The consistent behaviorist recognizes intelligence only in terms of the changing activities of the organism. Thus both mechanist and behaviorist wisely rid their technique of a troublesome anthropomorphic conception, even though they do not escape the necessity of explaining the complex reactions which the so-called intelligent organism displays.

But because the mechanistic and behavioristic conceptions avoid anthropomorphism, they ignore what is, after all, a distinctly anthropomorphic problem, namely, the problem of value. What we are interested in is an explanation of our individual and social values in terms of the instincts to which these

which have been attributed. Now the behavioristic method [illegible] eliminates the concept of value while the mechanistic method [illegible] renders the concept of value meaningless. The difficulty [illegible] the concept of intelligence [illegible] for its definition: intelligence is the ability of an organism to profit by experience. But this obviously [illegible] definition of intelligence leads us into a whole [illegible] literature. It precipitates us directly into the problem of consciousness, its origin, its significance. We cannot take up this controversy here. We shall here [illegible] a certain dilemma bearing upon our principle which any idea of intelligence gives rise to.

Intelligence [illegible] as an aspect of consciousness, may be merely a [illegible] observing the activities of instinct. In this case it can have no effect upon the instincts. Nor can it help the organism to profit by experience. On the other hand, intelligence may be a factor in the causal series which does enable the organism to learn by experience and to modify the original instincts. But if we admit such a factor, we admit the existence of something more fundamental than the instincts themselves. For how can the instincts, considered as mechanical entities, each giving rise to its characteristic value, combine to create a factor which shall represent the value of the organism as a whole? In so far as the instincts are defined as fixed and fundamental forces, it is impossible for them to create a principle to which they shall be subordinated. And on the other hand, in so far as we admit that the instincts are subordinated or harmonized in terms of the organism as a whole, to some other factor such as intelligence, we must give up the claim that the instincts are the fundamental forces.

IV. Emotion and Instinct

This criticism applies equally well to that type of psychological rationalism which regards emotion as the criterion of instinct. We may disregard entirely the obvious fact that as soon as we begin to define instincts in terms of emotions anybody's introspections would make a book. But even if it were possible to define certain instincts in terms of their emotional core,[12] we should still meet this difficulty, namely: in so far as we define the instinct-emotions as fixed we cannot account for the endless variation of instinct; and in so far as we account for the obvious varieties of instinctive expression, we discount the fixity of the instinct-emotions. It is because he fails to meet this issue that McDougall's account of instinct-values can be so comprehensive and at the same time of such little significance

[12]W. McDougall: "Social Psychology."

from a scientific point of view. Another writer could start either with half as many or with twice as many instincts as McDougall takes and attribute to them the values of society with equal plausibility and equal futility.

V. Summary

We may sum up the results of the discussion thus far very briefly. The mechanist and behaviorist define instinct in such a way as to eliminate the concept of value. On the other hand, those who define instinct in terms of emotion and intelligence make it so easy to attribute all values to instinct that the pursuit becomes everybody's game and nobody's science. Again where instincts are considered fundamental and fixed forces value cannot arise. Whereas where instincts are considered merely elements in an organism which expresses values more comprehensive than the separate instincts themselves, the big question is: What relations exist between the values which represent the organism as a whole and the instincts which go to make up that whole? In short, how can the conception of mechanism and value be logically reconciled?

VI. The Valuing Process

There is, in all of the views which we have discussed, an apparent failure to take account of a principle which has an extremely important bearing on the problem before us. This is the principle of synthesis or fusion, variously stated and applied by writers of the last century. John Stuart Mill, in his Logic, formulates this principle under the name "mental chemistry." It is the principle of G. H. Lewes' Law of Emergents, Wundt's Creative or Psychical Synthesis, and the governing principle of all those psychologists of the Meinong and Külpe schools who think in terms of 'Gestaltqualität'. Briefly we may state this principle as follows: The sum of the properties of any number of elements is not equivalent to the properties of any compound of these elements. Or, put in another way: The properties of any compound are unique and independent of the properties of the individual constituents.[13] A simple illustration of this proposition is the fact that hydrogen and oxygen, when united, produce a compound which has properties that neither of its constituents possessed before that union. In terms of our problem, this means that the elements or specific instincts

[13] I am indebted to Dr. Henry Hooker for the exact formulation of this principle. J. S. Mill, in his Logic, states it in various ways, as for instance: "The effect of concurring causes is not always precisely the sum of the effects of those causes when separate, nor even always an effect of the same kind." H. C. Warren's "History of Association Psychology" describes this principle historically.

which are supposed to enter into the structure of the organism do not, when taken together, explain the organism or the values which it may give expression to. An organism resembles a compound and has qualities not contained in any of the instincts, reflexes, or physico-chemical configurations into which it may be divided.

Now what are the properties which characterize the organism as a unit but which do not characterize any of its elementary parts? There may be many such properties; but may they not be summed up by saying that the chief characteristic of the organism as a unit is the valuing process? Is not this the really unique quality of an organism, and is not the organism in its various activities governed by this valuing process? We have already seen that instincts, taken as mechanistic or behavioristic entities, cannot really determine or give rise to values in any sense. They can merely tend to give rise to a certain more or less stereotyped response. The values which instincts apparently give rise to derive their significance solely from the relation between particular instinctive tendencies and the tendency of the organism as a unit. And the tendency of the organism at any particular moment is just this practical value judgment, whether explicit or implicit, conscious or subconscious, which uniquely expresses the impulse of any one instinct in terms of its organic setting.

Now, although we have explained this principle after a chemical analogy, it by no means follows that the term "mental chemistry," as stated by J. S. Mill, applies to the situation which we have in mind. For instance, according to the principle of fusion, chemistry is quite within its field in predicting from a compounding of hydrogen and oxygen a substance with the chemical properties of HOH. But HOH, in a situation involving the organism, has qualities or values quite independent of their chemical explanation. In relation to the organism, HOH may be water, a substance having the quality of wetness, and this quality is itself a factor which enters into the causal series and which determines the uses to which water shall be put. Just so, the actions of any theoretically isolated instinct may be explained in chemical, mechanistic, or biological terms, but in its organic setting it expresses itself uniquely in terms of organic values which themselves affect the operation of that instinct and which are not explained by the elements of which they seem to consist.

Therefore, the valuing process, through which the organism is continually expressing itself, is not to be regarded as an 'over-phenomenon' or as a static accompaniment of organic processes, but as itself a dynamic factor in the causal series; for it actually

enters into the determination of the instincts of an organism under certain conditions.[14]

It is the valuing process which brings about such radical changes in the instincts themselves, often causing the organism to act in complete contradiction of the stereotyped tendency of a powerful instinct. For example, we are told that the instinct of self-preservation and that of propagation are the most powerful and fundamental in the organism. And yet, we have numberless cases of individuals who have deliberately sacrificed their lives for values which they considered higher than life. Now it requires a strong stretch of the imagination, or a long process of rationalization, to explain how the instincts of life can give rise to values which exterminate the very instincts upon which they are supposed to be based, unless we recognize the validity of the principle which we have presented.

It may be answered that intelligence and reason are what enable the individual to subordinate the various instincts so that they will serve in the interests of the entire organism. Reason makes it possible for a person to see that his own life is less important than the life of society or the life of a friend. But what is reason without the play of a dynamic valuing process? Reason is essentially a playing against one another of a number of alternative values or value ideas, ending in the final adoption of one of them. What makes it possible to select one from a number of possible values is the presence, within the reasoning process, of a dominating value idea. It is the activity of this dynamic factor which gives to the reasoning process its significance, and which makes possible a choice of values quite at variance with the traditionally instinctive values.

It is really remarkable, once the unique character of the valuing process in an organism is grasped, to see how this conception completes or clarifies many of the problems which have been confronting us. Take, for example, the traditional division of the mind into cognitive, conative, and emotional states. Much of psychology has been concerned with an attempt either to distinguish or to reconcile these three faculties. Now, obviously, the fundamental factor in will, feeling, and reason is the selective factor, the valuing activity. Each of these three aspects of organic activity, therefore, is an expression, in slightly different form, of the valuing process. This is their common denominator.

In speaking of the valuing process as a common denominator it should not be thought that we are introducing into the complex-

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[14]A corollary of this, at least in principle, is J. S. Mill's statement to the effect that the generation of one class of mental phenomena from another does not supersede the necessity of an experimental study of the generated phenomena and their laws.

ity of organic and physical events the undue simplification of a mere word. For when we regard values as expressed by an organism, not as abstract mathematical functions nor as merely the mental side of a psychophysical parallelism, but as dynamic factors which themselves play an important part in the determination of instincts and the structural elements which make up the organism, we are describing a situation which is anything but simple. Indeed, our concept of value, properly interpreted, makes inevitable the conclusion that the phenomenal world is infinitely more complex than the deductions of many mechanists, behaviorists, psychologists (especially social psychologists) and others would lead us to believe. On the other hand, the valuing process, using the term as a class term, may consistently be regarded as the commom denominator in the explanation of all phenomena. For the valuing process, or some experience of value analogous to the simple experience that water is wet, is the point from which all reasoning and logical definition have their origin and to which, sooner or later, they must return. No matter where we look, no matter in what branch of science we may engage, the starting point or major premise is not a disinterested, logical conclusion, but an apparently arbitrary, certainly a highly interested dictum. But here again our assertion must be qualified by adding that, although value judgments are the logical limits of all scientific and philosophical reasoning, it by no means follows that the valuing process implies an absolute or unalterable standard of values. Values themselves are relative factors in a strictly relative series of phenomena or experiences. As such, they are subject to change either through a combination of unexplained circumstances or through the systematic investigations of science. To give a homely example, we may value beer as a healthy antidote for thirst; but the results of chemical and physiological investigations may later cause us to choose tea rather than beer. The very fact that values, which instincts are supposed to have produced, are subject to change in this fashion, still further complicates the problem of the relation between instinct and value.

VII. The Philosophy of Instinct and Value

Our study has now led us directly into the field of philosophical speculation. As a matter of fact, only a philosophical point of view can, at present, grasp the relation between instinct and value. And it is only philosophically that we can, at the present stage of the sciences, attribute values to instinct at all. This point will become clearer as we proceed. But even philosophy has struggled long and hard without becoming conscious of the

full significance of the valuing process. Philosophers have speculated all around the value idea, but have seldom realized in it the starting point of all philosophical speculation. The Greek philosophers made reason the distinguishing feature of man, but their emphasis on pure reason misled them. They failed to realize that reason was but a means of the human organism to an end; that the practical reason rather than pure reason was the characteristic aspect of man; and that the practical reason, out of which the pure reason grew as a tendril from a vine, itself proceeded only from the activity of the valuing process. The intellectual cast which the Greeks gave to philosophy has characterized it ever since, so that philosophy has developed largely into a problem of knowledge, an epistemology. In modern philosophy, the tradition of Hegel has reigned, and all life has been interpreted in terms of intellectual ideas, intelligent purpose, absolute knowledge, or completed meanings. It is therefore nothing less than astonishing to find how dependent even the most intellectual and disinterested philosophers are on the implied existence of the valuing process. Kant is, of course, the notable example. On the one hand there is the Critique of Pure Reason and on the other the Critique of Practical Reason. The moral imperative is a rather pompous but still easily recognizable exposition of the valuing process. Certainly, according to our point of view, it is the much more profound and enduring of his two philosophies, though most philosophers have considered it as something of an after-thought. On the other hand, Henri Bergson deliberately contrasts these two aspects of philosophy. The intuitive experience to which he gives priority more nearly resembles what we have described as the valuing process; though by interpreting life in terms of a vital force or an impulse rather than in the terms of a dynamic valuing process, Bergson was led into a serious and unnecessary contradiction between intellect and intuition, between intelligence and instinct, between the mechanical and the organic.

Bertrand Russell stands out among philosophers for his determination to eliminate the disturbing factor of value from philosophy and for his attempt to make philosophy a pure science. And yet Russell, at almost every turn, governs his theory by value-considerations. It is impossible to recount instances here, except to mention his analysis of 'hard' and 'soft' data, but an examination of his writings will reveal how continually this disinterested philosopher makes scientific decisions on the basis of a practical or value judgment rather than on strictly logical or theoretical grounds. Again and again he adapts his proof to the end in view, and as long as his theory enables him to construct in a fairly coherent manner those beliefs and values which he will not give up, he finds his theory satisfactory.

When we come to modern idealistic philosophers, we find that the valuing process is a key to many of the problems which they present. If we take, for example, Royce's analysis of an idea, we see an immediate connection with the valuing process. The significance of an idea, according to Royce, is what it intends. The difficulties which this philosophy encounters are clarified, even though not explained, when we see that at the root of all ideas is this dynamic valuing process. What an organism values may be imperfectly felt, and only faintly understood; still this value is bound to express itself finally in some coherent judgment. It is the value idea that "seeks its own."

As for Pragmatism, it has often been described as a method, not a philosophy. Our account of the valuing process provides a philosophic basis for Pragmatism. Why is it that immediate experience is the touchstone of all knowledge, morals, art? Because the significant fact about immediate experience is the valuing phenomenon. The sense of a dynamic value-idea constitutes both the given element of an immediate experience and the manner in which it shall work itself out in the thinking process. And when the Pragmatist asserts that the final test of truth is its workability, he implies the whole philosophy of the organism as a valuator. For every attempt at truth begins and ends with a concrete valuing judgment. And it is impossible to define workability except in terms of an evaluating standard. The organism and its unique qualities as an evaluator are therefore the philosophical concept upon which Pragmatism rests. As soon as the Pragmatist attempts to explain the significance of immediate experience in terms of instincts, reflexes, tropisms, sensations, etc., he falls into the abstract atomism of physics and chemistry, and loses the very thing he sets out to explain.

This and the philosophy of history are preëminently concerned with the question of value. The possibility of any system of ethics naturally depends upon the selection of a relatively fixed and generally accepted set of values. What most ethical philosophers fail to recognize clearly enough is that these values are never arrived at by a process of pure reasoning or by any scientific procedure. A system of ethics is built up only because the values which it arrives at were already actively present in the thinking process of the philosopher. Consequently we have a Paulsen whose highest value is energy: Aristotle chooses abstract reason: Kant chooses the moral law: the materialist chooses happiness or success. All these, with many minor variations, are clearly the result of the somewhat different personal values with which each individual works.

When we come to the interpreting of history and evolution, this phenomenon reaches its most dramatic significance. Why is it that practically every anthropologist and evolutionist con-

siders the present as the pinnacle of the past and survival as the test of supremacy? Why is it characteristic of historians to regard the events of history merely as the steps and errors by which the present high stage of civilization has been reached? Is it not because the values accepted by the living are the colored glasses through which they view the dead? The present is the highest or most valuable, not because it is an epitome of the past but because we, at this moment, actually value it most. Our values are not valuable because they have survived but because they are *our* values and because *we* value them. It is primarily because the survivor does the valuing that the values which survive are considered valuable. And it is because the surviving historian or the victorious nation *writes the history*, that the values which remain come to be considered the highest values.

VIII. Three Definitions of Instinct

We have now seen that the meaning of value does not lie in an analysis of instinct. Such an analysis but leads us further and further from the meaning of value. And yet it would be merely academic to insist, because instincts are not the fundamental determinants of value, that they play no part in its determination. We feel sure that the instincts do play a part, and a very important part. The question is, How and in what way? The answer to this question may be given in three parts, involving three separate and distinct definitions of instinct.

(1) *The Mechanistic Definition.* This definition has already been thoroughly described in the first part of this paper. Instincts, according to the mechanistic point of view, are definite physico-chemical entities which respond specifically to specific stimuli. The Loeb tropism, the chain of reflexes, the inherited nervous-muscular mechanism, are examples of instincts in the mechanical sense. Even the instincts sought for by the behaviorist, in so far as he is consistent, are mechanistic instincts. Now the mechanistic concept of instinct, we have pointed out, is extremely valuable as a method of research. It has notably enriched our concept of organic behavior and human nature. However, it is practically impossible to find instincts which satisfy the mechanistic point of view. Only certain lower organisms approach mechanical uniformity in their instinctive responses. And if organisms mechanically perfect could be found, then we should be unable to find any values whatsoever. For the very nature of value lies in the combination of imperfect organic mechanisms, conflicting tendencies, an unfavorable environment, and probably the presence of certain indefinable hereditary tendencies. It is this combination of a variety of things which gives rise to the active valuing process which is

probably the unique quality of an organism. We may say, therefore, that the mechanistic definition of instinct is a valuable working concept, but that instincts as thus defined have no real existence and cannot legitimately be used as an explanation of the phenomena of life in general.

(2) *The Pseudo-Scientific Definition.* However, the mechanist himself finds it impossible to adhere closely or consistently to a purely mechanistic conception, and so we find ourselves very soon in possession of a more flexible and less critical definition of instinct. According to this definition, the relatively distinct and specific ways of reacting to the environment are united into comprehensive tendencies which we call the principal or dominating instincts. The striking thing about instincts as thus defined is the fact that almost any instinct or organic mechanism can come to respond in an infinite number of ways to an infinite number of stimuli. This is the concept of instinct most prevalent today and most viciously used. It is characteristic of McDougall and many other psychologists. And it is the concept now made use of by economists, litterateurs, industrial men, and others who fondly think that they have found in psychology a simple answer to problems which have hitherto defied their attacks. This concept of instinct lends itself most readily to the manipulations of pseudo-psychologists and pseudo-scientists of all kinds. As James remarked—and it cannot be too often repeated—it is possible to make almost any classification of instincts on this basis, as long as it answers the writer's purpose.[15] And this is exactly what has been done.

Now we may believe with justice that instincts are important factors in the valuing process, but we cannot with any degree of scientific accuracy say that any instincts thus loosely defined are responsible for any particular values. We can romance about the subject. We can philosophize. We can psychoanalyze. We can rhapsodize. But all within very narrow limits. It is absurd for economists to believe that they can translate their study of economics from the field of conjecture to the field of science by borrowing a set of ready-made instincts from psychology. We can imagine Thorndike, Watson, McDougall, and a few others trying to agree on a classification of instincts as the axioms of economics! We may agree empirically upon the uniformity and strength of the sex instinct. But what relation has this instinct to the marginal desirability of a second, third, or fourth wife? We may believe in hunger as an instinct. But how does this instinct determine the marginal value of artichokes over cabbage, or chicken over duck? Self-preservation seems an obvious instinct and yet how does it determine

[15] "Principles of Psychology," ii, 382.

the marginal desirability of a dull, long-drawn-out existence over a short but exciting life?

If instincts, mechanically defined, can hardly be found existing, instincts according to the second definition can be manufactured so readily and so voluminously as to reflect seriously upon the mental process by which it is done.

(3) *The Popular Definition*. The third definition of instinct is the most vicious of all. Here instinct is practically synonymous with any mysterious manifestation of the organism. We hear of the religious instinct, the moral instinct, the instinct of service, the artistic instinct. Instincts on this level are responsible for such a wide range of values that they might as well not be responsible for anything. Like the benevolent monarch who, unknown to his grateful people, had been dead for twenty years, these broad instincts are but convenient names to which we can never attribute any specific value in any specific sense. We need only take the final step and call the valuing process itself an instinct in order to call attention to the absurdities into which such a loose definition of instinct can lead us. Such logic is undoubtedly what led Condillac to remark that instinct was nothing and Bohn to say that it was a mere word.

IX. Conclusion

Indeed, it requires an effort to keep the idea of value separate from the concept of instinct. Just because value is, in one sense, an unanalyzable factor, we tend to regard it as a sort of mysterious force, a sort of super-instinct which in some mysterious fashion governs and determines the lesser instincts. Undoubtedly, it has often seemed that this was the concept we had in in mind—a kind of over-soul, referee, independent valuator, Absolute, entelechy, which ruled the system of lesser values to which instincts gave rise. Nothing could be further from our intended meaning. Value, either as an idea or as a process, is to be regarded as a characteristic of an organism—a peculiar and unique quality, to be sure—but nevertheless a characteristic. We can but revert to our initial example of water. The unique characteristic of water is wetness, and more specifically, a wetness which only exists between certain degrees of temperature. This wetness is characteristic neither of hydrogen nor oxygen, any more than value is a characteristic of the instincts considered as entities. Now when the instincts—whatever they are—and the protoplasm, the germ-plasm, the effects of environment, are united in the form of an organism, the result is a new characteristic, namely, the valuing process. This process is not merely an intellectual spectator but an active, causal factor just as much as is the wetness of water. It enters actively into the

determination of instincts *and* habits, just as the quality of water is an active determinant of the uses to which it is put.

Therefore, there is in our concept of value nothing of the mystical or the absolutistic. Simply because value-judgments are in a sense the ultimates in our reasoning process, they are not put beyond the realm of knowledge and investigation. Because the present mechanistic conception of instinct is incompatible with the notion of value, we do not mean that there is an unbridgable gap between mechanism and organicity. And if the popular method of ascribing human values to instincts is fantastical or far-fetched, this does not mean that instincts in some way or other do not play a very important part in the makeup of values.

Although there are many conclusions to be drawn from our study, relating to value and instinct, the ones of most importance at the moment are two: first, that the vague and varied notions of instinct now prevalent have led to the wildest and most far-fetched conjectures as to the relation between instincts and values; and secondly, that as soon as we attempt to define instincts with care and precision, we shall be much more modest and scientific in the claims which we make for them. Such moderation and care will in the end lead to a much more accurate correlation between instincts and values than the elaborate cadenzas which psychological virtuosi are now improvising upon the general theme.

THE PSYCHOLOGY OF REFLEX ACTION

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I. *The Problem of Reflex Action.* Extremely anomalous is the position of reflex action in the domain of psychology. At least psychologists seem to be doubtful concerning the orientation of such behavior in the general psychological field and hesitant as to the exact attitude they should take toward reflex action. Traditionally, psychologists have been able to grant no more than that reflexes are exclusively physiological processes, because psychologists have always considered knowing as the essential datum of their science. To accommodate the fact that reflex action actually constitutes a part of the response equipment of organisms, as the biological influence upon psychology made apparent, reflexes, though not considered to be psychological phenomena, were tolerated as the motor accompaniments or the motor conditions of mental states.

In recent years a number of important conditions have conspired to bring about essential modifications in our attitudes toward reflex action, and especially influential in this connection is the fostering of the conviction that reflexes play a larger part in our total adjustments than was ever before realized. Prominent also among these conditions were the discoveries concerning the conditioning of reflexes. So recent have been these discoveries that at the present moment, lacking the necessary perspective, we are unable to realize precisely how great the changes are which they have effected in the domain of psychology, although it is apparent of course to everyone that important changes have taken place.

Another equally significant and by no means unrelated condition is the fact that psychologists in general are drifting away from the idea that a psychological datum is exclusively or even primarily a knowing fact in the sense of some psychic stuff or mental function, toward a more organic position. How great the change of front toward reflexes has been may be observed at a glance in the view now current and gaining ground that all psychological facts are based upon and developed from reflex action. Poignancy is added to this change of front when we reflect upon the great gulf which psychologists once considered to separate physiological behavior from elaborate knowing activities. As important as reflex actions undoubtedly are it

yet seems that we are going too fast and too far in our newer emphasis upon such behavior. Because there exists apparently so much uncertainty in the attitudes toward reflex action, the following study of reflexes is undertaken, with the aim of offering some suggestions toward the redefinition of these interesting and important types of behavior.

II. *Distinction Between the Psychological and Physiological Attitudes toward Reflexes.* Although, as we have endeavored to suggest in the preceding paragraph, psychologists have traditionally held themselves aloof from reflexes, because the latter were presumed to be entirely physiological, strangely enough it is owing in great part to the investigations of physiologists that the need for a closer study and understanding of reflexes by the psychologist has manifested itself.

How strange in fact it is that the physiologist's study of the conditioning of reflexes should induce the psychologist to recognize that a reflex action can and must be looked upon as a response to a stimulus, that is to say, an adjustment act, can be readily appreciated when we observe how great the differences are between the psychological and physiological attitudes toward reflex action. What is then the difference in the two attitudes? Merely this: that while according to the psychologist a reflex must be looked upon as a special adjustment of the organism as a whole, for the physiologist a reflex action is the operation of an autonomous system of particular parts of an organism. Now if this distinction is valid it is obvious that in order to reach an accurate description of reflex behavior this differentiation must be kept in mind.

Because the physiologist, while studying reflexes, is primarily interested in the functioning of neural structures, and secondarily in the activity of glands and muscles, he is disposed to look upon such behavior, as well as other types of responses which he studies, as constant mechanisms entirely independent of the surrounding stimuli. From this fact arises the distinction long current in psychological literature between the so-called physiological and sensation reflexes, the former being presumably completely autonomous and without the controlling influence of awareness. Accordingly the psychologist assumed that typical reflexes are exemplified by the visceral activities. Since on the whole, therefore, a reflex action for the physiologist consists of the innervating activity of a segmental neural apparatus, a limited extension and flexion of muscles, and the localized action of glands and nothing more, we must look upon the physiological description of a reflex action as an abstraction, wholly unsuitable for use by psychologists.

From the psychological standpoint, as we have suggested, a reflex action is a definite adaptation act and upon such a basis

is just as much a psychological datum as is thinking and knowing. Strictly speaking, the psychological organism under ordinary circumstances cannot act otherwise than as a psychological organism, and this refers to all activities, although for some purposes we might consider the individual performing isolated reactions such as merely digestion, etc. But these situations are exactly analogous to those accidental circumstances such as being struck by an automobile in which instance the individual may function as a mere physical object. To be entirely precise at this point we mean to point out that as a general principle, our exogenous reflex activities are stimulated to action by objects and events about us and operate as adaptational mechanisms in exactly the same sense as any psychological act.

In general, then, we may take as our standard for the differentiation between psychological and physiological reactions, a criterion which we verily believe to be in the main reliable, the question whether an act is or is not an organismic[1] response to stimulating circumstances. Now in order that an act should be considered a genuine organismic response we must be able to trace its arousal to some effect produced upon the person by some external object or some need for adaptation existing in the organism itself. Accordingly, as we might expect, no sharp lines of division mark off the internal from the external reflex stimuli. A food object operates precisely as does the hunger reflex (gastric contractions) in the arousal of salivary reflexes. Similarly from the standpoint of effecting an action in the person there is no functional difference between another person (opposite sex) and genital reflexes in the acting person when each serves as a type of stimulus to elicit (other) sexual reflexes. Whether the stimulus be endogenous or exogenous the reaction which it calls forth is an adaptation of the individual in just the same way that a habit or thought reaction is. On the whole, we find the reflex to be (and this is why it is a psychological act) an interconnection between organisms and specific things; or in better words, reflexes are the operation of reciprocally interacting stimuli and responses. Thus when I see a dish of apples my salivary reflexes may begin to operate, while a dish of peaches may not have the same effect at all. Now here the physiological attitude, according to which the problem of the reflex begins and ends entirely within the organism, contrasts with the psychological study in that the latter is concerned with (1) the means whereby the end-effect, which is the secretion or muscular contraction, is initiated, and (2) how the act is dependent upon the reactional characteristics of the specific individual performing the act. The psychologist cannot afford to overlook the fact

[1]The term organismic refers to the absolute inseparability of the response and stimulus factors in a psychological reaction.

that reflexes are very strictly conditioned and that upon the type of stimulus object which elicits the reaction depend the intensity and range of the behavior.

Persuasive as an argument for the organismic character of reflex action is the testimony of the physiologist himself. Asserts Sherrington, than whom no investigator is more qualified to speak in this matter, that reflexes are fractional pieces split off from an animal's total behavior which are artificially though conveniently treated apart[2], and that a simple reflex is probably a purely abstract conception, a convenient if not a probable fiction.[3] Furthermore, even the experimental physiologist finds it necessary to declare "that the reflex reaction cannot be really intelligible to the physiologist until he knows its aim."[4] And so the physiologist considers as an essential part of the investigation of reflex phenomena the eliciting of their purposes. This does not mean at all the indulgence in any factually baseless speculation, but merely involves looking upon a reflex action as a fact in its adaptational perspective. When the operation of a reflex mechanism occurs, it is necessary in the interests of a fair understanding of it, to include as many as possible of its essential features. Among such essential features we may mention the influence upon the reflex action of the location of the stimulus—the local sign of reflexes, as Sherrington calls it.

If the experimental physiologist acknowledges what we are pleased to call the definite psychological character of reflex action, certainly the psychologist may well pause to reconsider his habitual descriptions of such behavior. Let us hasten to add in unequivocal terms that to adopt the psychological standpoint of studying reflex action means not at all that our study will lose one iota of its objective character. On the contrary, such a method of study will add completeness as well as definiteness to our descriptions. In plainer words, the psychological standpoint implies that we shall look upon the reflex response as well as upon every other act that falls within our purview, as the adjustment of a psychological machine, in the sense that we shall correlate the acts of the organism with the coincidental surrounding conditions.

Is it necessary to add, in view of our discussion and our calling to witness the experimental physiologist, that there is no actual conflict between the physiologist and the psychologist? No such conflict exists in fact, since each worker is merely interested in a different phase of the same series of events. While the psychologist is interested in the total action of the person to some definite stimulus, the physiologist is interested

[2] Integrative Action of the Nervous System, 1911, 237.
[3] Ibid., 8.
[4] Ibid., 238.

in the workings of the reflex mechanism as they operate within the organism itself. What we are desirous of showing is that when the psychologist is satisfied to duplicate the work of the physiologist then he cannot hope to do justice to the psychological facts in the case.

If our distinction between psychological and physiological behavior is valid, we find in it a compelling warning not to confuse reflex behavior with the truncated activities of injured and partially destroyed organisms. All experimental animals such as the decerebrate pigeons of Flourens and Schrader, Goltz's dogs and other laboratory animals exhibit atypical forms of behavior which cannot be fairly taken as examples of reflex responses. How valid this point is will come out later in our discussion of the differences between normal responses of animals and human beings.

III. *The Nature of a Reflex Action.* From the psychological standpoint, then, we must look upon a reflex action as a specific sort of behavior segment and this means, as we have indicated, that we must not only investigate the response mechanisms but the stimulating circumstances as well. Now the special reflex characteristic of the reflex type of behavior segment is that there is only one reaction system in it. To be more specific the reflex activity, although the adjustment of a complex animal or person, is a simple and immediate final response to a directly presented stimulus. Obvious it is then that there are no precurrent or anticipatory reactions in reflex segments of behavior such as we find in our complex behavior segments, in which the final act is preceded not only by a definite attention set but also by another reaction which we may call a free perceptual or ideational act, and still other sorts of responses.[5] It is the absence of the anticipatory or precurrent responses which justifies the statement that reflexes involve no foresight of the end or knowing by the organism with respect to what is to take place before the response occurs or what is in fact transpiring at the moment of action. But what of the complicated reflex behavior in which apparently several adaptations are taking place? Upon investigation we find as a matter of fact that such behavior can be analyzed into a series of behavior segments; that is to say we can analyze the behavior into a series of stimulus and response coordinations. And here, as is not the case elsewhere, we have a chain effect. One final response serves as the stimulus for the next reaction and so on throughout the series no matter what its length.

From the unitary character of the response in the reflex segment of behavior follows the fact that reflex action is abso-

[5]For a detailed analysis of a reaction system see an article in the Journ. of Philos. 1921, 18, 253.

lutely unintelligent action. And this statement holds true no matter how complexly conditioned is the response act. For the character of intelligence is an essential contribution of the precurrent reaction systems. The latter are means of conditioning actions so that they can serve to adapt the organism in a very precise way to a particular total stimulating circumstance. Needless it is to contrast this prescient type of conditioning of one part of a response by another phase of the reaction, with the simple conditioning of a total reaction by the stimulus, as is the case in reflex behavior. The most complex conditioning of the latter sort, while demonstrating an awareness on the part of the reacting organism, merits in no degree the ascription of intelligence.

As a consequence of the unitary character of the reflex response it appears to possess the following specific characteristics, namely (1) relative automaticity, (2) constancy, (3) permanency, and (4) localizability.

(1) *Relative Automaticity*. Since there is but one immediate movement or one essential secretory act, then the reflex response must perforce appear as practically automatic. An evidence of the automaticity of the reflex is the fact that it occurs and reoccurs in practically the same way no matter what the person is doing at the time or where he is when the stimulus is presented. It is obvious, of course, that when the reflex behavior segment is connected with other segments the total behavior situation of the person may appear different, although the individual reflex act remains the same. Generally speaking we might consider that the mere presence of an adequate stimulus, whether primary or secondary, will throw the reaction system into automatic operation.

It is needless to add perhaps that the automaticity of reflex action is relative in the sense that no psychological reaction can be wholly without spontaneity. What is meant of course is that relative to other responses the reflexes are immediate and direct consequences of the stimulation. There is, in fact, a very close relationship between the stimulus and the response, and no variation through interpolated responses is possible between the appearance of the stimulus object or situation and the final adjustment.

(2) *Constancy*. The constancy of reflex actions is a fact which follows from the function which they perform in the various adaptation situations of the organism or person. Reflex behavior of the simpler sort adapts the person to the simple maintenance situations in which he is found, such as shielding ourselves from immediate noxious stimuli and nourishing ourselves in order to grow. Note that in the trophic reflexes, for example, the mere presence of the food objects at certain strat-

egic points of contact with the organism (at pillar of fauces, for deglutition) brings about the action; also in the shelter reactions, the pin prick, the hot or cold object must be in immediate contact with the organism. Now all of these food and shelter conditions are constant factors in the surroundings of the individual and consequently the reflex adaptations remain constant in their functional and morphological character, although as we have intimated, in the human being reflexes may become organized with other behavior segments. It is possible also that the reaction system as a whole in reflex behavior segments may become slightly modified because of changes in the size and tonicity of the organic apparatus, although the general character of the reaction remains constant.

(3) *Permanency*. Since reflexes are elementary forms of responses adapting the organism to permanent specific conditions they are permanent factors in the reactional equipment of the organism. Moreover, reflex reaction systems do not become integrated and modified to become phases of larger and more complex reaction systems. They remain simple reflexes. We have already indicated that complex reflex adjustments consist of numerous repetitions of a particular reaction system of which the preceding members of the series serve as stimuli to the following ones. In short, a serial reflex is merely a series of behavior segments and not an integration of reflexes into more complex behavior.

(4) *Localization*. The comparative simplicity of reflex reaction systems and the definiteness of their operation permit us to look upon them as partial acts. As a result, it appears as though the organism operates in limited segments when functioning reflexly. Thus we speak of an eye or hand reflex. This partial functioning is not an actual fact, however, for it is a biological and psychological impossibility for the organism to act unless it acts as a whole. When we withdraw our hand from a hot object with which it accidentally comes into contact we obviously react as a complete organism. Similarly every reflex no matter how great a change it produces in the person's relations to his surroundings may be for practical purposes circumscribed and localized in a comparatively limited area.

IV. *The Analysis of a Reflex Reaction System*. Since from our standpoint the reflex reaction system is a typical example of the ordinary unit of psychological activity it would be unnecessary to single it out for analytic description were it not for the fact that reflexes are frequently and always fallaciously presumed to be different in principle from other forms of behavior. To us it hardly seems possible that such a difference should exist and as it is entirely unlikely that any asserted dif-

ference will lie in the glandular, muscular or neural mechanisms involved in reflexes and other action types, we may therefore confine our analysis to what would conventionally be called the mental phases or accompaniments of reflexes.

Let us be understood then, as forthwith declaring that whatever factors are present in psychological responses of whatever description are found also in reflex behavior. And if psychological phenomena may properly be partitioned into cognitive, conative and affective factors, these factors are found in reflexes no less than in any other behavior segments.

(1) And first let us consider the cognitive factor. Every reflex action involves a definite discrimination of stimuli, although the discriminative factor is more pronounced in some reflexes than in others, a condition, however, which reflexes share with all types of psychological behavior. If evidence is needed to prove the presence of a cognitive element in reflexes we need only refer to the fact that in common with all psychological responses, reflexes require their specific adequate stimuli to put them into operation. A hot object will call out the reflex, while a warm object will not produce such an effect. Again, the conditioning of reflex behavior constitutes excellent testimony to its psychological character.[6]

What precisely is cognition then? It is necessary to specify that by cognition we refer to the fact that different objects elicit differential responses from the reacting person or organism. Clearly, in the case of such comparatively simple responses as reflexes the differential reactions will be aroused not so much by complex objects as by simpler qualities of such objects, or in many cases the differential response may be elicited by a condition rather than by any specific quality.

Obviously, we must all agree with those who assert that when we perform a reflex reaction we do not know just what is taking place, for in such a reaction there is lacking the verbal response systems which among other factors strikingly represent the knowing element. This absence of overt knowing, however, in no sense militates against the fact that a reflex action is a differential reaction or a cognitive process. As we are planning to indicate in a later section of this paper, the entire general prejudice against regarding reflexes as psychological processes,

[6]Probably this point does not require much emphasis since we find in a definitely mentalistic textbook the following footnote: "The purely physiological reactions are not absolutely divorced from consciousness. It is a demonstrated fact that mental states may influence or even initiate these physiological activities. The perception of food may start the secretion of saliva. The presence in the mind of certain ideas may affect circulation and respiration—the bated breath, the blush of shame, the pallor of fear, the flush of anger, all testifying to the effect of consciousness upon the purely physiological activities." Breese, Psychology, 1917, 398.

as well as the particular bias against looking upon reflexes as involving cognitive factors, have their roots in the acceptance of an unsatisfactory conception of cognition. This conception implies that knowing is something separated from the adjustmental act. We believe that all difficulties involved in the ascription of cognition to reflex action are dissipated when we recall that by such action we mean in the final analysis an adaptation of the complete organism.

(2) In similar fashion we may find in reflex action an affective factor also. Otherwise stated, an analysis of such behavior reveals a change produced in the organism which we may well call a feeling condition. Glandular and visceral activities are aroused indicating that not only has the stimulus object been acted upon by the organism, but also that the action has extended to itself. Here again it is entirely superfluous to suppose that in any sense the person overtly reports to himself that he feels thus and so; the feeling situation or affective response is merely a sort of internal response involving relatively more the visceral organs than the external skeletal muscles. Naturally the diffusion and intensity of the visceral disturbances will correlate with the violence of the response to the urgency and pressure of the stimulus.

(3) In much the same manner can we analyze in reflexes the conative factor, by which is meant the attention change from one stimulus to another. In no reflex action, of course, is there present the deliberate precurrent change of position or attitude by which the individual prepares himself for adaptation to a new stimulus. Hence, if the term be allowed, the attention factors in all the various reflex action systems are involuntary, that is to say the person exhibits more or less violent jerky movements in shifting his adjustments to new stimuli.

Once more we repeat that throughout this entire analysis of reflex reaction systems we refer to the behavior of psychological organisms or persons. To those of our reactions which are merely biological responses, namely tropisms, and we cannot well doubt that we occasionally perform such behavior, these descriptions which we have offered do not at all apply. In the interests of accurate description we cannot be too careful at this point, for since the psychological organism is obviously a biological organism as well, it consequently is sometimes, albeit very seldom, thrown back upon what we must call biological or tropismic modes of response.

V. *Reflexes Are Not Neural Mechanisms.* If our description and analysis of reflex behavior segments are corresponsive with the facts in the case, then it is manifest that our interpretation of reflex action is in conflict with and must replace the prac-

tically universal belief that these forms of behavior are merely specific forms of neural acts or circuits. In clearer words, the essential thing about a reflex action is supposed to be a particular concatenation of neurones usually described as preformed patterns in the nervous system. Probably the most fundamental error in the neural theory of reflexes is that the neural apparatus is in some sense presumed to be the cause of the muscular movement and glandular action which constitute the observable results of the reflex action. In the neurone theory apparently the neural circuit replaces the soul or consciousness as cause of a given adaptation. Credence is lent this view when we consider that as a matter of historical fact the so-called spinal reflex was sometimes considered to be the exclusive reflex type of action, while at other periods the spinal reflexes were presumed to be the typical if not the exclusive reflex responses.

In addition to the general difficulty which is involved here, of neglecting most of our reflex reactions, for we probably have as many cerebral as spinal reflexes, another question arises equally fundamental for the whole of physiological psychology. Does the neural apparatus control the muscles any more than the muscles and glands control the neural apparatus? Is it not a fact that the specific pathways involved in any reaction are involved because certain muscles or glands need to function? The writer is firm in his disbelief in the functional priority of any system to any other. In fact, to our way of thinking no such priority exists or can exist in the ordinary circumstances of behavior.[7] Especially clear becomes the problem of the supremacy of the neural apparatus when we consider the activities in which the muscle spindles are the primary receptors, for in such activity we have a circular process from which it is extremely difficult to analyze out prior or posterior phases of the reaction. This action in our opinion minimizes the general view of the primacy of the neural apparatus in any type of reflex action.

Is it not closer to fact to affirm that the neural, muscular, glandular as well as all other action phases of any behavior are simultaneous in their functioning and that no system is prior to or more important than any other? What actually happens in every psychological behavior is that the organism performs an act of which all the component systems are phases, in the sense that they constitute factors of a total response. In their aggregate these phases constitute an adaptation to some object or situation. But apparently we have dissipated the cause of the adjustment. What, it is asked, if not the neural

[7]In various experimental procedures we may of course consider either the neural or muscular acts as prior.

apparatus, conditions how the muscles, glands and other phases of the reflex should act?

To this we answer: what in fact could be the cause of which the total (neural, muscular, glandular) adjustment is the effect but the stimulus object or situation, for in general what other observable causes of our actions are evident? Again let us stress that a reflex action represents a differential mode of behavior, neural, muscular, glandular, etc., which the organism has acquired in the course of its development and which now operates when its adequate stimulus is presented. The failure of reflex descriptions is largely owing to the fact that psychologists do not recognize the dependence of reflex action upon actual stimulation, exactly as is the case in any other psychological action. When we do appreciate the relationship between the stimulus and the total unitary response then we can cheerfully dispense with the causative character of the neural apparatus.

Now here another objection may be anticipated. How can we argue that the neural factor is not primary in importance nor prior in time when we know that as a matter of fact the essentially conductive function of the neural apparatus requires some time to operate, no matter how brief? Such an argument, we reply, rests upon a misconception which can be obviated by a closer observation of facts. Of a surety when we experiment upon a neuro-muscular coordination we may with perfect propriety disengage logically the conducting from the contracting mechanism, but when we do perform such a logical analysis we must not forget that such an experiment implies the falsism that the mechanisms are both inactive at the inception of the experiment, when in fact both nervous and muscular mechanisms are functioning before the experiment is started, since the organism is never at rest. Further, this objection implies that a single neural impulse can be in fact isolated and that it can be traced from a receptor to a muscle or gland. Now it is incontrovertible that a psychological organism is constantly in action and therefore neural impulses are discharging uninterruptedly over all the tracts in synchronous harmony with muscular, glandular and other types of processes. What in actuality happens when we present the organism with a stimulus is a redistribution of action, an emphasis of other features of the person than were prominent when the new stimulus appeared, in short there is a refocussing of the individual upon a new stimulus.

By way of emphasizing our hypothesis that if a reflex action is a psychological datum it is a segment of behavior, i. e., a stimulus and a response, we mean to deny as stated before that a reflex is a partial reaction in any sense. In especial, we mean to controvert the three typical forms of the decurtation theory

[illegible] and phy- [illegible] not primarily neural [illegible] the operation [illegible] activities of [illegible] they merely neuro- [illegible] we return to the [illegible] adaptations to [illegible] and circumstances.

[illegible] abstractions by the [illegible] question exists at [illegible] a unified whole.[9] [illegible] experimental [illegible] indicate, physi- [illegible] the factitious [illegible] appears most [illegible] presented in the [illegible] contrary are ser- [illegible] persist in their [illegible] sees them only as [illegible]

[illegible] physiology we be- [illegible] organismic [illegible] decerebrate [illegible] in parts. [illegible] animals is that [illegible] simple activ- [illegible] descriptions [illegible] when we forget [illegible] and its seat. [illegible] action in [illegible] dimness.[11] [illegible] that the [illegible] them without [illegible] must we [illegible] that the ani- [illegible] is mental [illegible] experiments can [illegible] the intrinsic [illegible] reason, exper- [illegible]

[illegible] of Neurology, 1920, [illegible] is not even a [illegible] Human Physiology, [illegible]

[10] For an [illegible] sensorial [illegible]

[11] Luciani, [illegible] com- and psychic [illegible] plexity?

iments on the higher apes or the human being produce either "shock" or death. But as long as the functional organization of the animal remains undisturbed throughout all the mutilations it is as much a psychological organism as it ever was. No other view would ever have been held but for the assumption by most workers that consciousness was a force or power separate from, but paralleling exclusively, the cerebral functions, or was co-ordinate with other neural functions, as Pflüger, Goltz, and Lewes believed.

We find in the reflex controversy,[12] as well as in the facts which the contending parties sought to interpret, considerable evidence for our contention concerning the unitary character of psychological behavior. Both the mechanical and spontaneity arguments are of course partial views, as the facts employed in them amply testify, and are not nearly as much descriptions of those facts as they are metaphysical interpretations. Both views are frankly based upon a psychoneural dualism, the existence of which we unqualifiedly deny. That the organismic theory is sound may be further seen from the fact that in the human organism much reconstitution and substitution of behavior may occur during and because of the degeneration of cortical tissue,[13] although the extreme intensity of the general functional organization is such that few liberties can be taken with the individual because of destroying the animal's organization.[14]

VI. *The Origin of Reflex Behavior*. From the standpoint of genesis, reflex responses are unique among the permanent behavior equipment of human beings, in that they may be considered as the earliest and most intrinsic of all the types of responses. The simplest of them are organized and operate considerably before the completion of gestation. Exactly does this fact comport with the function and general behavior conditions of these comparatively simple but utterly essential activities. Reflex behavior is essentially life-maintaining activity and therefore is most intimately related to and dependent

[12]Cf. Lewes, The Physical Basis of Mind, 1895, Problem IV.

[13]Franz, Cerebral-Mental Relations, Psychological Review, 1921, 28, 81; Variations in Distribution of Motor Centers, Psychological Review, Monograph Series, 19, No. 1; Brown and Sherrington, Note on the Function of the Cortex Cerebri, Proc. Physiol. Soc., Mar. 15, 1913; Journ. of Physiol., 1913, 46, xxii.

[14]Excellently illustrated is the highly integrative character of the human responses by the phenomena which Head and Riddoch (Brain, 1917, 40, 188-263, 264-402) have called mass reflexes. It appears that when the human cord is transsected the reflex responses of the individual which are found after the shock subsides are greatly reduced in functional complexity and effectiveness. Naturally we cannot agree with Rivers (Instinct and Unconscious, 1920, 28) that these reflexes are "suppressed" actions.

upon the biological structures of organisms. Hence, reflexes absolutely must begin to operate from the very inception of the organism's life; in fact the reflex reaction systems may be said to be inherited if this can be said of any response system along with the specific organs which have a part in their operation. So elementary and primary are some of the simple reflex responses that as a general rule the impression is prevalent that all reflex reactions are congenital and that none of them are acquired in the life of the person. Such an assumption is not strictly correct. To account for the essentially adaptive character of such basic and undeveloped reactions which are not inappropriately named protective, defensive, avoiding, and seeking responses we must fall back upon some sort of natural selection hypothesis.

Need we add that neither the mode of origin of reflexes nor their essential features of permanency and constancy deprive such responses of any specific psychological character? Let it be noted that the psychological domain comprises behavior covering wide ranges of complexity and effectiveness, but significant is the point that all these types of reactions are determinate responses to stimuli whether the adaptation involved be complex and imply much previous contact with its stimulus or whether the reaction be fairly simple and occur while the organism is in primary contact with the stimulus calling out the act in question.

It is possible that the basic character and primitive origin of the reflex responses contribute no small share to the constancy and permanency of these reactions. That is to say, as long as the type of organism remains unmodified and as long as the reciprocal stimulating circumstances remain the same, then there is no need for the variation in the response system.

VII. *Distinction Between Human and Animal Reflexes.* Because of the comparative simplicity of reflex action it is doubtless true that the slightest variation exists between human and infrahuman behavior at this point. And yet if we were to overlook the enormous differences that after all exist between human and animal reflexes we should do irreparable damage to our observations as well as our interpretations. For there are great differences even between the various reflex actions of the human individual, depending upon size, weight, health, and maturity, which cannot be neglected in any analysis of behavior, especially if we are to attain exactitude in our descriptions. From the existence of the different reflexes in the human species it follows that there must be extreme variations in the behavior of the individuals of the human and infrahuman developments.

In support of the proposition of the wide variation between human and infrahuman reflexes two sorts of considerations

suggest themselves. First, not only the general biological differences between the two species of animals but also the specific neural disparity argue conclusively for great diversities between the two types of reflexes. The second and more striking consideration is the general fact of behavior equipment. If our hypothesis is valid that a reflex act is a specific type of organismic adaptation, then clearly the behavior must be colored by the total reactional condition of the individual. In other words, the reflex action in the human being must be a function of the entire behavior equipment of the person[15], and the specific surrounding circumstances. Now obviously the behavior equipment of the person is so different from that of any infrahuman animal that the reflex behavior as well as any other class of action will be very different in the two cases.

Whatever argument is offered for the continuity of the two series of reflexes must perforce be based upon continuity in biological development. Now such continuity, it must be observed, does not imply any similarity in specific acts of psychological behavior; rather the argument for continuity overlooks all actual facts of concrete behavior in favor of a general developmental or descent hypothesis. Such a neglect of the specific adjustment inevitably results in error. To illustrate, it was only because of a lack of interest in actual adjustments that the believers in continuity attempted to make of spinal reflexes the typical reflex action to the exclusion of cerebral reflexes, and moreover, they believed this in disregard of the fact that even when reflexes are considered as neural mechanisms they are cortically controlled and modified.[16] In view of the cortical control of reflex action who can deny the distinction between reflexes which we are attempting to make? In concluding this section of our paper we might suggest that our distinction between human and infrahuman reflexes in no wise interferes with the biological continuity doctrine. For the logical implication of our hypothesis is that only a degree of difference exists between any two levels of psychological action. In consequence, it is our argument that animals are not different from human beings in lacking memory, thought and language, as the textbooks would have it, but only in the capacity to respond with simpler memory, thought and language reactions. But note, that in all cases of behavior the needs of psychology dictate a careful and accurate differentiation and description of responses.

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[15]How potent is the behavior equipment of the organism in influencing its actions may be well observed in the illustration which Whitman quotes of the pigeon reared by a wholly different species which will reject a mate from its own species in favor of the species under which it was reared. Cf. Orthogenic Evolution in Pigeons, vol. 3, The Behavior of Pigeons, 1919, 28, edited by Harvey Carr.

[16]Cf. Herrick, Introduction to Neurology, 1920, 68, 120, 312.

VIII. *Types of Reflex Action.* For practical purposes we might classify reflex reaction systems into at least five types, partially upon the basis of their organization and especially upon the kind of contact which they effect with their ordinary stimuli. In general, reflexes may be adjustments to conditions (1) within the organism, or (2) to changes surrounding the individual or (3) to both of these at once. The first type we may name the interoceptive reflexes and we may mention as illustrations of such responses the stomach and intestinal reflexes, etc., or expressed differently, responses in which these phases of the organism play a prominent part.

On the other hand, reflexes which are primarily adjustors of the person to outside stimuli we may call exteroceptive actions. Here we may analyze two types which we will name localized and general exteroceptive reflexes respectively. In the former type the response appears to be localized in a definite way and involves primarily external skeletal mechanisms. As examples of these reflex actions we may name the hand, foot or body withdrawal responses to heat or pain objects, the knee jerk, turning the head toward a flash or sound, etc. The latter type, i. e. general reflexes, contrasts with the local responses in that a larger phase of the organism is saliently involved and also in the fact that the visceral and glandular factors may dominate the segment of behavior. As examples of the general exteroceptive reflexes we may quote writhing, trembling, shivering, etc.

The third class of reflex which adapts the individual to both external and internal stimuli we will call combination reflexes; these we may likewise analyze into two types, local and general. The former type would comprise adjustments of a more or less restricted sort, although on the whole the reactions would be more complex than those in our second class. Among the localized combination reflexes we may enumerate the sexual and salivary responses. In this class both the local and general responses may involve much glandular activity although the latter involves so much more of the visceral and glandular factors that we may refer to some of them at least as feeling reflexes. Illustrative of the general combination reflexes are the "startle" and "start" responses which are frequently confused with feeling and emotion reactions, and which in some cases constitute the simplest form of attention acts.

It is plain, of course, as we have indeed suggested, that the specific reaction systems in these different types of reflexes will be integrated from specific factors. For example, the principal interoceptive reflexes as a rule will include mainly both muscle and glandular factors while the exteroceptive reflexes involve primarily the skeletal muscles. Again, practically all

the interoceptive and some of the exteroceptive reactions will involve the sympathetic nervous apparatus in a prominent way, while the exteroceptive reactions will involve mainly the central nervous system. A prominent exception to this rule is the iris reflex in which the muscles involved are innervated by the sympathetic system. In such comparatively simple reactions as reflexes the discriminating factors would naturally be, as we have already seen, the simplest found in any reaction. In all of these cases, to be sure, the discriminating factor strictly speaking is nothing more than the occurrence of a simple differential response to its specific stimulus-object. As in all cases of classifying reactions the divisions and subdivisions that we have made represent only attempts to order behavior and not the separation of unequivocally different responses. For this reason no classification can avoid many overlappings and the value of any classification may be judged most adequately by the criterion of whether it suggests the likenesses and overlappings of behavior types or whether it serves to obscure such similarities and transcursions of the classes.

IX. *The Stimulation and Conditioning of Reflex Action.* A radical change in the view concerning what constitutes the stimulus for reflexes is implied in the acceptance of the organismic hypothesis. For if we assent to the view that reflexes are adjustments of the individual some of which are very complex, then we can no longer entertain the notion that they are aroused to action by merely simple thermal, light or sound radiation. Aside from the general confusion which this notion implies between the media of stimulations and stimuli objects or situations,[17] such a view in the domain of reflexes excludes all but the simplest situations as stimuli.

Let us notice then that reflex action is stimulated as are all other kinds of responses by objects of various sorts, and by circumstances and situations. To be plainer, human reflex actions are rapid and localized responses to things, persons, and conditions. Now this way of describing the reflex situation allows for the fact that the whole person is acting and not a single part of him, which is of course an impossibility. Moreover, this mode of analysis forestalls the tendency to overlook any type of reflex response, since we may be entirely certain that the class of reflex action is large enough to include more than the very simplest avoidance responses. How complex and varied the stimuli for reflex actions actually are appears clearly in the consideration that objects in particular settings will elicit

[17]For a distinction between stimuli and their media, cf. Kantor, A Tentative Analysis of the Primary Data of Psychology, Journ. of Phil., 1921, 18, 257.

the salivary reflexes while the same objects in different settings or different objects in the same settings will not bring out the reflex adjustment. What more complex stimulus can there be than the actions which operate while we are reflecting upon or reading of some person or event which excites us to sex or hunger functioning? Extremely informing also are the observations concerning the reflex changes in the person while under the subtle social and sex stimulation of other persons. Especially important here are the complex social objects and situations which constitute the stimuli for intricate human reflexes of all sorts. As among such social situations we may refer to games and gatherings of persons of the same or opposite sex which arouse sex and hunger responses of various degrees of diffusion. Again, we are familiar with the revulsion responses which dead or live animals produce in us when touched or seen; these are all complex reflex responses representing functions of the total reaction equipment of the person to customary stimuli which are therefore social in nature.

In the last mentioned reflex adjustment as well as in many others we meet with the very important conditioning activities influencing the adaptation of the person to his surroundings. Thus, for example, the nauseous visceral responses to dead animals may have become definitely attached to this new or accessory stimulus at some specific time and under particular circumstances. The early stages in training an infant to perform proper excretory behavior is in great part a process of attaching reflexes already present and functioning to a new eliciting stimulus.[18] Especially subject to the conditioning process are the combination reflexes, since the internally stimulated act can be variously transferred to and from the coordinately stimulating external object. So involved are the conditioning processes that in many cases it truly appears that the reflexes have become integrated into more complex forms of behavior, although as a matter of fact this type of response remains practically in its original condition throughout all of its complication by attachment to various new forms of stimuli.

X. *Reflex Action as Stimuli and as Behavior Setting.* So intimately related are the reflexes with the total behavior of the organism that they constitute the stimuli to many of our reactions. Because of this intimate relation, however, the reflexes are frequently overlooked and their importance unsuspected. As a consequence psychologists are frequently guilty of the assumption that mysterious powers bring about various reactions, whereas a careful study reveals that the reactions in

[18]This conditioning process may antedate the development of the necessary inhibitions.

question have definite stimuli in the reflex responses. There can be little doubt that reflex stimulation is responsible for much of our action which we call diffuse feeling-responses and moods and that they compose elements in our complex social acts; acts of love, pity, revenge, etc. may be due in large measure to reflex stimulation.

To state it otherwise, the reflex stimulations comprise some of the facts referred to as mixed motives in complex responses. Can we deny that it is through the accessory stimulation of reflexes, in addition to thoughts and memories aroused by tales of cruelty and violence, that we are induced to add our contribution to alleviate the suffering of which the stories inform us? In further illustration of reflex stimulation we may quote the ways in which our reflex responses color our reactions to aesthetic objects. Thus through the operation of these by-play reflex reactions we are stimulated to read human and personal qualities into physical objects stimulating us. In many instances the additional stimulation by reflexes may also supply energy and alacrity to the behavior. This latter fact has been celebrated in the statement that artistic production is the sublimation of sex impulses. While the term sublimation hardly represents a psychological process, still it does serve to emphasize the prevalence and power of reflex stimulation. In all these cases, reflexes serve as adjunct or additional stimuli and as a result of their operation the individual may act in an entirely different way than if he were stimulated only by the original object. Suggestive it is to note that the way we respond reflexly to things and persons in turn stimulates our first impression reactions to those things and persons. In other words, reflexes of a complex sort are called out in us by things and people and then when we attempt to formulate a judgment of these things and people, or if we merely are prepared for some future response, the delayed action is based and dependent upon reflex responses.

Furthermore, reflexes may function as combination stimuli-responses in delayed reactions, which function in general is made possible by the fact that series of reflex responses intervene between the presentation of the stimulus and the final adjustment, and thus function in many cases to keep alive the effect of the stimulation until the final response occurs. The combination reflex act-stimulus is undoubtedly a primary basis of the wants and desires as psychological facts. One is forcibly led to this view by the consideration of the place of sexual and hunger-digestive reflexes in food and sex wants and desires. So potent are the reflex actions as stimuli, that we can elicit from a careful study of the differential frequency and intensity of such reactions much valuable information concerning the type differences between individuals which are usually consid-

ered as temperament, disposition, etc.; such characteristics being marked by actions which in a large measure are stimulated by reflex responses.

And finally, the visceral and glandular reflex behavior of persons may serve not as direct stimuli to actions but rather as the setting of responses. That is to say, these reflex responses serve as influences of behavior affecting the general condition of the person. As the setting of stimuli, visceral reflexes determine whether or not certain stimuli shall be potent and call out a reaction. Thus for example, during the operation of hunger reflexes it is more difficult than otherwise to attend to one's work; in other words, the sensitivity to exacting stimuli is decreased; in other cases the functioning of reflexes may serve to increase one's sensitivity to particular stimuli and in consequence the person will respond more readily to surrounding objects.

XI. *Reflex Action and Instinctive Behavior.* Very prevalent is the view that reflex actions are closely related to instinctive behavior and especially is this assumption made in order to provide a solid foundation for instincts. In detail, instincts are presumed to be combinations or chains of reflexes, and since the latter are supposed to be specific neural pathways the conventionally teleological instinct achieves a factual support. From our standpoint, however, the relationship requires reformulation, since we cannot assign to instincts any sort of teleological character nor can we consider reflexes to be merely neural mechanisms.

Instinctive actions we consider to be behavior segments of a different order and type than those of reflexes and the main difference is that the instinctive behavior segments contain more than one reaction system.[19] Common to both instincts and reflexes, however, is the fact that both acts constitute definite final adjustments although an instinct segment of behavior contains a pattern of several reaction systems. Because the instinct segment of behavior does contain more than a single reaction system, it partakes of a series of definite characteristics not found in the reflex segments. These characteristics we may enumerate as follows: (1) spontaneity and variability, (2) modifiability, and (3) integration.

(1) By spontaneity and variability we mean to refer to the greater adaptability which instinctive behavior exhibits than do reflexes. The latter may be conditioned in various ways; that is to say, the simpler reaction system can be differently attached to a stimulus, but the response factor itself does not vary. In

[19]Unquestionably the fact that these reaction systems are morphologically exactly like those of reflexes constitutes the factual basis for saying that instincts are chains of reflexes.

the case of the instinctive reaction on the contrary, the members of the pattern may rearrange themselves in modification of the pattern.

This arrangement is made possible by the fact that while the total pattern may be stimulated by a definite appropriate stimulus, say some interoceptive reflex, the individual reaction systems in the pattern are aroused to action by other surrounding stimuli, namely, objects and conditions; so that each has some autonomy and the whole pattern is spontaneous. Accordingly, the instinctive behavior act is the more adaptable response when the adjustment conditions are more variable. This type of instinctive adjustment in which the member reaction systems are subject to rearrangement is the most spontaneous that we can observe. The reactions which are less spontaneous are so because the member reaction systems of the segment are only very little stimulated by the surroundings and more by the preceding members of the pattern series. It was this kind of instinct, no doubt, which gave rise to the notion that instincts are chains of reflexes. When there is only one, or at most only a few surrounding stimuli conditioning the instinctive behavior, then the reaction as a whole will be more rigid and conform to a type.

(2) Since the individual reaction systems in an instinct behavior segment are correlated with specific external stimuli it is entirely probable that the auxiliary stimuli may become more and more effective as factors in the total response, thus modifying in a specific way the total instinct act and making it more serviceable to the organism in its particular surroundings. This modifiability contrasts with the permanence of the reflex behavior segment which cannot of course be modified in any essential degree.

(3) Another intrinsic characteristic of instinct acts is the fact that when conditions allow and make necessary they become integrated into more complex reactions. It is this fact of integration above all which marks off the human from the infrahuman instinctive behavior and also distinctly differentiates instincts from reflexes. While the reflex activities remain practically as they originally appear, instinct behavior becomes developed into more complex forms of responses. And so it happens that in the animal domain the instinct reactions become integrated to only a slight degree because the conditions are not conducive to any considerable development. In the human individual on the other hand, the behavior conditions are so complex that there are very few instinct responses to begin with and these few become integrated into larger reactions and disappear. By the same token the reflexes which originally comprise

elements in the behavior equipment of the individual remain with him in practically the original number and condition. We might repeat here once more that reflexes become modified only by conditioning, that is to say, the correlated stimuli may vary and consequently the whole behavior segment becomes altered but not the response factor or reaction system itself. It is consequently clear that in our comparison of reflexes with instincts, animal instincts must be understood to be the facts discussed, while in the case of reflexes both animal and human actions may be considered as the subject matter in question.

Superfluous it would seem now to add that in our entire discussion we referred not in a single sentence to instincts in the sense of a purpose or impulse in the individual to do various kinds of acts; no such impulses, we firmly believe, exist in any sense.[20] Many times we have implied that we are discussing only action mechanisms which are constituted exclusively of definite adjustment acts conditioned by stimulating circumstances with which they are coordinated.

XII. *Reflexes and Tropismic Action.* As a final consideration of reflex action we may place it in comparison with tropismic or purely biological action. Since there is no strict convention governing the use of the term tropism, let us be understood to exclude the criterion, that such action does not involve systematized nervous tissue. For our hypothesis concerning the adjustments of organisms does not permit us to seek in the mechanisms of organisms for the exclusive conditions of behavior. Now observe that very prominently is the comparatively simple mechanism of the tropismic action correlated with the sensitivity of the organism to its surroundings. As a matter of fact it is possible to differentiate between reflex actions as typical psychological responses, and tropisms, on the basis of the relationship of the organisms to the surroundings when they are performing either one or the other type of action.

Although it is entirely probable that the difference between tropisms and reflexes is merely a variation in developmental complexity, still we can specify particular adjustmental differences. For example, tropisms as responses, while entirely disproportional to the exciting condition in the expenditure of energy, in form and type of movement are still constant. This constancy of movement is a function of

[20] But this is no sufficient reason, however, for doing away with the name instincts as some recent discussions of instincts seem to imply, since we believe the term may reasonably be used to symbolize some definite form of psychological phenomena.

definite organic structures operating as a whole in the manner which is referred to as irritability, and correlates exactly with some stimulating condition in the surroundings. This external condition is practically an undifferentiated condition and never an object with its specific qualities. Moreover, the reactions are such as maintain the present status and condition of the individual's organization exclusively by means of metabolic functioning. From this standpoint it is easy to see why we must look upon tropismic action as relatively simple responses to surrounding conditions.

Reflexes as psychological activities, on the other hand, are specific responses to particular objects and conditions. Such behavior, as we have seen, constitutes differential responses and exhibits a subtle interdependence of stimuli and responses. By virtue of the person's possession of numerous characteristically different reflex action systems to which many different specific objects and conditions are coordinate, the individual is selectively sensitive to many features of the *milieu*. That this essential psychological character of reflex actions has been overlooked may be accounted for by the fact that many of the specific differences between reflexes and tropisms seem to fall away when we compare them both with the higher developed and more complex adaptations which we may call volitional responses. Such an overlooking of the definite psychological character of reflex action is exceedingly unfortunate, of course, since as a matter of fact reflex responses can be shown to partake in some fashion of practically all the typical characteristics of the complex psychological reactions.

XIII. *Conclusion*. In conclusion, we might suggest that our study of reflex action finds its most important feature in the general psychological problem which it raises. How shall we look upon psychological phenomena? Shall we consider them as definite autonomous facts in nature or shall we look upon them as merely epiphenomenal attachments to such facts? Or, again, shall we try to make psychological facts into physiological actions, because presumably psychological activities are not concrete or simple enough to handle without changing them into neural terms? In our study the conclusion we reach is that unless we consider reflexes as well as every other type of reaction as definite psychological facts, and not physiological acts, we cannot hope to understand them. That psychological acts are just as definite and just as real as any other kind of fact investigation has amply revealed. A definite criterion for a psychological fact we have discovered in the intricate interconnection between a stimulus and a total reaction of an organism.

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FUNCTIONAL PSYCHOLOGY AND THE PSYCHOLOGY OF ACT: II.[1]

By E. B. Titchener

§5. We have now to set a new scene. For while the psychologist of function works, as we have shown, in a biological atmosphere, the psychologist of act, with whom we have next to deal, lives and moves in an atmosphere of logic or theory of knowledge. Functional psychology—so we might say, twisting Fechner's famous phrase—is a psychology from below, a psychology to which we work upward from the more fundamental science, and the psychology of act is a psychology from above, to which we work downward from the superior discipline of logic.

'Function,' Ladd remarks, is "a vague and sufficiently indefinite term,"[2] and the statement holds, unfortunately, of biology as well as of psychology. 'Act,' on the other hand, is a term which, whether it occur in logical or in psychological context, may be defined with some rigour. If we cannot frame a definition at the outset, the fault lies not with any ambiguity of 'act' itself, but with the multiplicity of contexts in which the technical term appears. We shall do best to proceed chronologically, and thus to obtain materials for a retrospective survey.—

The importance of the 'act' in modern psychology derives from the work of Brentano. And we may begin by quoting the sentences in which Brentano distinguishes psychical from physical phenomena.

"Every psychical phenomenon is characterized by what the scholastics of the Middle Age have termed the intentional (or, sometimes, mental) inexistence of an object, and what we (although the expressions are not wholly free from ambiguity) should term reference to a content, direction upon an object ('object' not meaning here a 'reality'), or immanent objectivity. All alike contain within them something as their object, though they do not all contain the object in the same way. In idea something is ideated, in judgment something is accepted or rejected, in love something is loved, in hate hated, in desire desired, and so on.

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[1]Continued from this Journal, xxxii., 1921, 519 ff.

[2]*Philos. of Mind*, 1895, 300.

[illegible]

however, challenged, and on substantially the same ground, by Meinong (1899) and Husserl (1901). These critics point out that the 'content' and the 'object' of act, which are identified by Brentano, must in fact be kept apart. When I perceive a house, for example, I most certainly am not ideating my sensory contents (Brentano's physical object). I am rather ideating, by and through these sensory contents, a transsubjective object, namely, the house in question. Brentano's concept of 'immanent objectivity' is therefore not adequate to a descriptive psychology.[9]

The effect of this criticism presently appears in the psychological writings of the Meinong school. Höfler (1897) remarks that, while a theory of knowledge must discriminate content and object, psychology, whose object is always immanent, may dispense with the distinction.[10] But Witasek (1908) draws a less simple picture. Every elementary psychical phenomenon is now taken as twofold or two-sided, as at once act and content. There can be no act without content, and no content without act; the distinction is no more partitive than that of the colour and spread of a surface or the velocity and direction of a movement. Logically, however, the separation may be made, and psychologically—act and content are equally 'psychical'—separate treatment is, within limits, convenient.[11]

The 'content' of a psychical phenomenon is the 'part' whereby it brings a determinate object to consciousness, and the 'act' is the 'part' which makes the object an object of perception or imagination or judgment. The essential character of the psychical, the character that marks it off from the physical, is accordingly this reference to a transsubjective object, a transeunt reference to something beyond itself. In Witasek's own words:

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[9]A. Meinong, "Ueber Gegenstände höherer Ordnung und deren Verhältniss zur inneren Wahrnehmung," *Zeits. f. Psychol. u. Physiol. d. Sinnesorg.*, xxi., 1899, 185 ff.; E. Husserl, *Logische Untersuchungen*, ii., 1901, 344 ff. (esp. 353), 396 ff., 694 ff. A little later comes T. Lipps, *Leitfaden der Psychologie*, 1903, 53 ff., 139 (1906, 5 ff.; 1909, 8 ff.): cf. also "Bewusstsein und Gegenstände," *Psychol. Untersuchungen*, 1905, 1 ff., and "Inhalt und Gegenstand: Psychologie und Logik," *Sitzungsber. d. kgl. bayer. Akad. d. Wiss.*, 1905, 511 ff. The general statement of the text applies more closely to Husserl than to Meinong, who still speaks of immanent objects. Husserl, of course, recognizes the limiting case in which object and content coincide (333, 337 f., 352, 363, 376, etc.). But it is not my purpose at this point to enter into details. Nor have I thought it worth while to try to carry the distinction of content and object further back. Höfler's claim ("Sind wir Psychologisten?" in *Atti del V. Congresso Internazionale di Psicologia*, 1905, 327) will hardly hold water, in view of his own statements in the *Logik* (1890, 7) and the *Psychologie* (1897, 3): cf. Husserl, 470.

[10]A. Höfler, *Psychologie*, *loc. cit.*

[11]S. Witasek, *Grundlinien der Psychologie*, 1908, 73 ff., 280 ff., 318 f.

"My ideating, my thinking, my feeling and my willing are always in their own peculiar way 'aimed' at something. I ideate *something*, a something that is not the ideating, perhaps a book; my thinking grasps things that are not themselves thinkings, indeed, that do not belong to the mind at all; it grasps them, without in any way drawing them into itself; there is, and there can be, no suggestion of a spatial relation; and yet my thinking 'seizes' those things. The same thing holds of feeling and of willing. A relation, truly, that would be mysterious, nay, inconceivable, if we were not so familiar with it from our inner experience! But it is altogether confined to the psychical. Examine the physical, search the world of material things, as carefully as you may, and you will find not a trace of it. You will find relations of space (inside, outside, alongside), you will find movement to and from, you will come upon all manner of relations: but this—this intrinsic reference to, direction upon, pointing toward something else—has no place among them....Here, we may believe, is the most tangible, most characteristic difference between the two spheres."[12]

Psychical phenomena still form a class of their own, separate and distinct from physical phenomena. But for Brentano the psychical phenomenon is an act, in which a content or object (which is primarily physical) is intentionally contained. For Witasek the psychical phenomenon is an act-and-content, whose nature it is to point to some object (very often a physical object) that lies beyond it.[13]

The elementary phenomena of Witasek's system are, on the 'intellectual' side of mind, ideas and thoughts, and on the 'affective' side, feelings and desires.[14] Brentano's loving-hating has thus been subdivided.

§7. At the same time that Meinong and Husserl criticise Brentano's definition of act, Münsterberg (1900) objects that the specified acts are not logically coordinate. Brentano, as we saw, gives priority to the act of ideation: "[all psychical phenomena] either are ideations or.....rest upon ideations as their basis."[15] Münsterberg argues that this ideation is not an act at all. An act is an attitude of the subject, an attitude in which we say Yes or No to a presented object or content.[16] Judgment, for example, covers the paired opposites of acceptance and rejection, affirmation and negation, and interest the paired opposites of loving and hating. But where is the activity, the Yea-saying or the Nay-saying, in the case of ideation? "We speak

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[12] *Ibid.*, 3 f., 74.

[13] I am not quite sure of this interpretation. Witasek seems to say expressly (75) that the reference of the psychical phenomenon is an affair of act, and therefore not of content; and all the terms employed in the quotation just given are, as my rendering shows, names of act, and not of total psychical phenomenon. Yet the content is psychical (5, 74).—For the physical object, cf. *ibid.*, 6, 12, 73 f.

[14] *Ibid.*, 81. Witasek's doctrine of processes and dispositions does not here concern us.

[15] *PES*, 111; cf. Höfler, *op. cit.*, 3 f.; Witasek, *op. cit.*, 97. Husserl's discussion (*op. cit.*, 399-463) will engage our attention later.

[16] H. Münsterberg, *Grundzüge der Psychologie*, i., 1900, 19 f.

of an Ideating," declares Brentano, "whenever something appears to us (*wo immer uns etwas erscheint*)."[17] The implication is that we are brought indifferently, apathetically, into the ideational state, and in that event there can be no question of an 'act.'

Münsterberg would, undoubtedly, have urged the same objection against Witasek's system. For here, also, we find a recognition of the Yes-No attitude in all the elementary phenomena except ideation: in thought (affirmation and negation), in feeling (pleasantness and unpleasantness) and in desire (wanting and spurning.)[18] Witasek declines to give a formal definition of ideation, and contents himself with examples. He sums up his discussion, however, in the remark that ideas, the wholes of act-and content, are "so to say the psychical copies (*Bilder*) of the objects with which our consciousness is occupied."[19] We need not press the language, but we get, again, the suggestion of indifference on the part of the subject. Moreover, Witasek asserts categorically that "the antithesis of Yes and No is altogether incommensurable with ideation."[20] In what sense, then,—so Münsterberg might have asked,—is ideation an 'act'?

The criticism is telling: but it hinges, of course, upon the definition of act. That is a matter to which we shall presently return. Meantime, it is interesting to note that the objection has been turned by a writer who could not admit its validity. It is turned by Stumpf, in his doctrine of 'psychical functions.'

§8. Stumpf finds that the 'immediately given' comprises three irreducibles: phenomena, by which name he denotes sensory and imaginal contents; psychical functions, which include such activities as perceiving, grouping, conceiving, desiring, willing; and the immanent relations between and among functions and phenomena.[21] All functions (with the exception of the primitive function of perceiving) have, further, their specific correlates or contents,—forms, concepts, objectives, values,—

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[17] *PES*, 261; cf. 106.

[18] *Op cit.*, 80, 280, 353.

[19] *Ibid.*, 97 f.

[20] *Ibid.*, 308; cf. *PES*, 291. This statement of Witasek's forbids us to read anything like selective attention into the phrases whereby he characterizes ideation: "[das] sich Präsentieren eines neuen Inhalts" (78), "[die] Vergegenwärtigung eines Gegenstandes" (98), etc. Attention, in fact, is treated both by Höfler (*op. cit.*, 263 ff.) and by Witasek (297 ff.) in the section devoted to Judgment. Stumpf originally followed the alternative road left open by Brentano and made attention a Feeling (*Tonpsychol.*, i., 1883, 68; ii., 1890, 279). Brentano himself would apparently (*PES*, 263) have taken a like course.

[21] C. Stumpf, *Erscheinungen und psychische Funktionen*, 1907, 6 f.; *Zur Einteilung der Wissenschaften*, 1907, 5.

which Stumpf calls collectively formations.[22] Phenomena, relations and formations, as objects of thought, give rise to the three neutral sciences of phenomenology, logology and eidology. These *Vorwissenschaften* taken together may, if we care so to apply the term, be named theory of knowledge.[23]

We have passed over the psychical functions, which come to their rights in another way. Since relations are common both to functions and to phenomena, the 'immediately given' shows an intrinsic duality.[24] We are led by it, though not directly, to the distinction of psychology and natural science. Phenomena form the starting-point for both,—the logically necessary starting-point for natural science, the empirically necessary for psychology.[25] The proper subject-matter of psychology is, however, to be sought in the psychical functions.[26] Throughout our actual experience, these are continually and closely connected with phenomena.[27] Stumpf insists, nevertheless, that the connection is not logically necessary. Though every function must have a content, the content need not be phenomenal. Moreover, even as empirically conjoined, functions and phenomena are independent variables. Their assignment to different sciences is further justified by their radical difference; they have no single character in common, unless it be the character of time.[28]

Psychical functions are also called acts, states, experiences.[29] Stumpf distinguishes, with Meinong and Husserl, between the content and the object of an act, but his distinction is differently worked out. An object is a conceptual formation. Hence an act that stands below the level of conception cannot have an object. In bare perceiving, for instance, we have phenomenal or relational contents, but no object. Conversely, when our thought is directed upon the universal as such, upon concept or law, content and object coincide; the content is, by its very nature, object. Between these limits stand all the cases in which we are occupied with a general or invariant (object) on the basis of a particular or variable (content).[30]

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[22] *Gebilde psychischer Funktionen*: cf. *Erscheinungen*, 28 ff.; *Zur Einteilung*, 6 ff., 32 ff.

[23] *Zur Einteilung*, 26, 32, 38, 40.

[24] *Ibid.*, 6, 10.

[25] So I interpret Stumpf. Cf. the definition of natural science, *ibid.*, 16, and the *notwendig*, *ibid.*, 6.

[26] *Ibid.*, 20; *Erscheinungen*, 6, 39.

[27] *Erscheinungen*, 7, 27, 38 f.

[28] *Ibid.*, 11 f., 15.

[29] *Ibid.*, 4 f.

[30] *Zur Einteilung*, 6 ff. I hope that I here express correctly the relation of *das Zentrale, die Invariante* of the conceptual formation to its casual accompaniments. Cf. also *Erscheinungen*, 16 ff.

What, then, are the psychical functions? Without professing to make out a complete list, Stumpf distinguishes two great classes, the intellectual and the emotive, and names certain functions under both headings.[31] On the intellectual side, the most primitive function is that of perceiving or remarking or taking note of: it includes the two modes of sensing and ideating.[32] Another fundamental function of the intellectual life is comprehension or grouping (*Zusammenfassen*), whereby "a number of discriminated particular contents, impressions of touch, lines, tones, can be combined into a whole, a figure, a rhythm, a melody".[33] Next follows conception (*das begriffliche Denken, die Bildung von Allgemeinbegriffen*), and last in order stands judgment.[34] On the affective side we have such paired opposites as joy and sorrow, search and avoidance, willing and rejecting.[35]

Here, where we have Münsterberg's objection in mind, we are especially interested in the primitive intellectual function of perceiving, which replaces the 'simple ideation' of Brentano's system. Perceiving or remarking is an intrinsically analytic, as comprehension is an intrinsically synthetic function.[36] From the standpoint of classification it would therefore be simplest to bracket these two functions together, and allow the opposition of analysis and synthesis to replace, in the ideational sphere, the Yes and No of the other functions. Stumpf himself seems to recognize such an opposition in the instance of conception.[37] Yet we are told that perceiving is 'most primitive,' so that it takes precedence of comprehension. Moreover, it is clear that the function has, by its nature as analytic, a negative as well as a positive implication of its own. While we are taking note of a part or attribute, while there is "an accumulation of consciousness over against"[38] this part or attribute, we are necessarily failing to take note of all the rest of the presented whole, from which consciousness to the same degree recedes or is withdrawn. "The barest act of attending or heeding," says Münsterberg, "is of itself an act of subjective evaluation, near akin to emotion, and fundamentally different from mere ideation."[39]

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[31] *Erscheinungen*, 5, 7.

[32] *Ibid.*, 16.

[33] *Ibid.*, 23.

[34] *Ibid.*, 24 f.

[35] *Ibid.*, 26 f.

[36] *Ibid.*, 16 ff., 23.

[37] *Ibid.*, 25. Yet it is noteworthy that the illustrative list (7) runs: "*Zergliedern, Zusammenfassen, Bejahen und Verneinen, Begehren und Ablehnen*," without a connecting *und* between the first two terms.

[38] *Ibid.*, 17.

[39] *Op. cit.*, 20.

[illegible] and neglect. [illegible] There [illegible] different [illegible]

[illegible] admit the validity [illegible] he attributes [illegible] certain [illegible] But if [illegible] among [illegible] more im-portant [illegible] functions [illegible] psycho-logy upon [illegible] logical [illegible]

§ [illegible] requirement is met by a system which affirms the [illegible] of the subject, the I of consciousness, [illegible] Such a sys-tem is that of Lipps,[c] which we proceed briefly and partially[d] to analyse.

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[a] [illegible] 1907, 22: [illegible] XXV. [illegible]

[b] [illegible]

[c] Lipps' *Leitfaden* appeared in three editions [illegible] The second edition, which is the most systematic of the three, is largely different from the first, and the third [illegible] differs in certain important respects from the second. Concurrently with these editions Lipps published *monographs* and articles—expository, constructive, polemical—in which various phases of the system are worked out in greater detail (cf. the selected bibliography by G. Anschütz, Arch. f. d. ges. Psych., XXXV., 1915, 13). He was, indeed, continually revising and correcting, expanding and experimenting, so that a later work is not only an improvement upon an earlier, but is also itself the starting-point of new insights and arguments. For this reason his psychology, as we shall see, never attained to systematic completion.

[d] In particular, we are *not here concerned* with Lipps' views of explana-tory psychology.

Psychology, Lipps tells us, is the science of consciousness and of the experiences of consciousness (*Bewusstseinserlebnisse*).[44] The peculiar function of consciousness is to reach out, beyond itself, into a world transcendent to it; this "jumping over its own shadow," as Lipps put it, is the very essence of consciousness.[45] The experiences are of various kinds.

Lipps begins by differentiating sensations, as 'objective,' from all other conscious experiences, which are 'subjective' or experiences 'of me.' Sensation is the mere 'having' of a sensory 'content.' This 'having' is a 'running against' or 'happening upon' (*Widerfahrnis*); the sensation is a 'receptive experience;' the content of sensation is given only to the 'eye of sense.'[46]

If now I turn my 'mind's eye' upon a content, I pass from receptive experience to an experience of 'activity.'[47] The term is technical: the experience of activity is a line or stretch of consciousness, which begins and ends with punctiform 'acts.'[48] In turning my mind's eye upon the content I start, so to say, with an act of 'hello!'; this act is drawn out into an activity of attention or apprehension (*Auffassung*); and the activity comes to an end, 'snaps to,' with an act which Lipps names the 'simple act of thought' (*den schlichten Denkakt*), whereby I disengage an object 'for me' or 'over against me' from the original content 'in me.'[49]

With the appearance of objects, consciousness becomes more complex. Not the 'eye of sense,' and not the 'mind's eye,' but the 'eye of intellect' (*das geistige Auge*) is henceforth in function.[50] I start with the simple act of thought, which is drawn out into the activity of 'apperception.'[51] But apperception is of two

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[44]T. Lipps, *Leitfaden der Psychologie*, 1909, 1. The term *Bewusstseinserlebnis* has not settled down to its final definition. (1) An *Inhalt* is not an *Erlebnis*, but an *Erlebtes* (3). But in 1906 (3, 355) *Inhälte* were *Erlebnisse*. So in the index of 1909 (391) they still figure as *eigenartige Bewusstseinserlebnisse*. (2) In 1906 (8, 27) acts were *Erlebnisse*. In 1909 (21, 23) acts of thought are *Erlebnisse* only when their imaginal contents are adequate to the objects of thought, and acts of conation are never *Erlebnisse*, since we do not experience the objects upon which they bear. Yet the older mode of speech is sometimes retained (*e. g.*, 40). One does not see why acts (quite apart from the I-experience involved) should not be *Tunerlebnisse* (cf. "Das Ich und die Gefühle," *Psychol. Untersuchungen*, i., 1907, 693): but Lipps was apparently on the track of a new distinction which he had not thoroughly worked out. Meantime we must accept contents, acts and experiences as in strictness irreducible.

[45]*Ibid.*, 12.

[46]*Ibid.*, 7, 16, 20, 23, 27.

[47]*Ibid.*, 26.

[48]*Ibid.*, 22.

[49]*Ibid.*, 23; 14 f., 22, 25 f, 141 ff; 13, 25; 9, 12, 13, 25 f.

[50]*Ibid.*, 8, 13, 25.

[51]*Ibid.*, 22, 25 f., 144 ff.

kinds: classifying and questioning. If it is of the classifying kind, I end it either by a simple act of 'fixation' of the intellectual eye, an act which constitutes my object a single, determinate, particular object; or else, passing beyond these acts of bare fixation, I bring it to a close by some act of comprehension, relation, abstraction.[52] If apperception is of the questioning kind, a further complication arises. The objects which I question reply to me; they have their own status and their own laws, in virtue of which they lay their pretensions or claims before me. And I may just 'listen to' and experience these claims, or I may acknowledge them. If I acknowledge them, the activity terminates in an act of judgment. In this case there is a direct parallel between apprehension, with its terminal act of thought, and apperception, with its terminal act of acknowledgment.[53] If, on the other hand, I only listen to them, I have what Lipps calls an 'experience of claim' (*Forderungserlebnis*). This is a receptive experience, and therefore akin to the 'having' of a sensory content. It is a feeling of dispositional tendency, of compulsion or constraint.[54]

The experience of claim plays a large part in Lipps' system. For the moment, however, we leave it aside, in order to characterize the 'acts.' These are punctiform 'doings' of the conscious I, and may occur either independently, as 'empty or naked' acts, or in connection, as the initial and final points of an activity. Lipps distinguishes various sorts of acts. There are acts of ideation (that is, of productive imagination), of thought, of conation, of judgment. 'Wishing,' for instance, is a naked act of conation. But since conation, whenever circumstances permit, extends from act to activity, we have acts which institute or inaugurate this activity, acts of impulsion, incitement, urge, and acts which round off the activity, acts of arriving, completing, succeeding.[55]

The mention of conation brings us back to the *Forderungserlebnis*. Consciousness or the conscious I, whose essential nature we have seen to consist in self-transcendence, is also identified by Lipps with activity;[56] and activity is always an act of conation (*Streben*) expanded into a conscious stretch.[57] But conation is itself the 'subjectified experience of claim:' it is, so to say, the resultant of two sets of tendencies, the tendencies imposed on the 'I' by objects, and the tendencies, directions, pressures, needs, resident at the time in the 'I,' or it is a claim's

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[52] *Ibid.*, 26, 149 ff.
[53] *Ibid.*, 26 f., 30; 11, 31; 32 f., 189 ff.
[54] *Ibid.*, 31, 33, 34.
[55] *Ibid.*, 21 f., 33; 22; 21, 22, 23, 32, 42, 263, 296.
[56] *Ibid.*, 6, 39.
[57] *Ibid.*, 23.

'effective resonance' within me. If the tendencies are in accord, the experience is that of active conation; if the imposed tendencies run counter to the resident, it is that of passive conation. In any case, conation and activity are definable as the interrelation or cooperation of an object, with its claim, and the individual consciousness.[58]

If, however, consciousness is activity, what becomes of the 'receptive' experiences, and more especially of the 'having' of a sensory content? Lipps meets the difficulty by his doctrine of 'potential' activity. To 'have' a sensory content is to have it 'in my power.' I feel that I 'can,' if I so desire, turn toward it, direct upon it my activity of apprehension: or rather, since activity presupposes an object, that I can direct my apprehension upon the object implicitly or potentially contained in it. This distinction of actual and potential activity, though it is psychologically irreducible, is still a distinction within the general experience of activity, and thus guarantees the essential likeness of receptive and active experiences.[59]

To round out this summary account, we must say a word of two further classes of conscious experience. The one of these comprises the feelings proper, the affective feelings, which are 'states (*Zuständlichkeiten*) of the I,' 'colorings' of the activity that is consciousness.[60] The other includes the experienced relations. The resolution of consciousness into a series of acts and activities does not destroy its unity and continuity. For the acts and activities bring with them experiences of conditioning and being conditioned, of dependence, of procession or issuance; in experiencing them, we also and at the same time have experiences of 'motivation,' that is, of their relation to other conscious experiences.[61]—

Not everything in this account rhymes or, as Lipps might say, 'snaps to;' the thought is not of the kind that can properly be reduced to tabular form. We have, however, gained a basis for our special question: are the acts and activities two-faced, positive and negative? The answer seems, without a doubt, to run in the affirmative, though there is no evidence that Lipps offers it as the formal answer to a question of doctrine.

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[58] *Ibid.*, 34 ff., 261 ff. Similarly in the acts of thought there is a "peculiar interrelation between the I and the objects," which makes the acts at once creative and receptive (21 f.).

[59] *Ibid.*, 14, 28 ff., 39.

[60] *Ibid.*, 37 ff., 40, 314 ff.

[61] *Ibid.*, 40 ff. In 1906 Lipps affirms that "just as conation and activity are an echo or a reflection of the claims [of objects] in the individual consciousness, so is the interconnection of conations and activities by motivation a reflection of the interconnection of claims" (29). This doctrine, and with it the reference to *Zusammenhang der Forderungen* in the index, have disappeared in 1909. Yet 1909, 300 repeats 1906, 266.

Conation, he says,—and we remember that conation is fundamental,—conation is "positive and negative, endeavor and resistance, wishing and wishing-not, willing and willing-not. The relation between the two is analogous to that between the consciousness of validity and the consciousness of invalidity, or between the positive and the negative judgment."[62] This passage recognizes the two-sidedness of activities (of activity or conation in general and of the activity of willing in particular),[63] of acts (wish and acknowledgment),[64] and of potential activities. For the consciousness of validity and invalidity belongs to the experience of claim, which is a receptive experience, like the having of a sensory content;[65] and this 'having' itself is a 'having in my power' to turn toward or to turn away from.[66] We may add that feeling, the 'tingeing' of our activity, shows the same dual nature; we find the antithesis of pleasant and unpleasant, large and small, familiar and strange, and so forth.[67]

Everything, therefore, except the contents[68] and the experiences of motivation, has the Yes-No character which Münsterberg demands.

§10. We began this discussion with the pioneer work of Brentano, and we have used the criticisms of Meinong and Husserl and of Münsterberg as pegs upon which to hang an account of certain act-systems. We have thus been able to set forth, so far as is necessary for future comment and comparison, the systems of Witasek (as representative of Meinong's school), of Stumpf and of Lipps. The central point upon which these psychologists agree is that consciousness is by its very nature intentional, that it transcends itself and refers to objects beyond

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[62] *Ibid.*, 260 (1906, 230; 1903, 203 f.). Lipps might have added to the negative judgment the negative perception and the negative recollection (201 ff., 212 ff.).

[63] *Ibid.*, 301 ff.

[64] *Ibid.*, 36: Lipps speaks of a feeling "des Anerkennens oder Abweisens, des Fürwahr- oder Fürfalschhaltens."

[65] *Ibid.*, 31.

[66] *Ibid.*, 13 f.

[67] *Ibid.*, 37 f., 314, 329, 332, etc.

[68] Lipps was not writing with Münsterberg in mind. For he compares the antithesis of pleasant-unpleasant with that of light-dark in the domain of color-contents (*ibid.*, 37, 314). The parallel is only casually drawn, but Lipps found it possible. In the account of the sensory contents (69 ff.) there is, of course, no hint of any *Gegensatz*.

it.[69] The word 'intentional,' however, reminds us that our survey is not yet complete. By the side of Meinong, Stumpf, and Lpps we must place a fourth writer,—one who is not a psychologist, one indeed who believes that there is a great gulf fixed between his own science and psychology,[70] but one who has, nevertheless, exerted a profound influence upon current psychological thought.

Stumpf reserves the term 'phenomenology' for the science that deals with sensory contents and the corresponding images. Husserl's phenomenology is neither this phenomenology of Stumpf's nor is it identical with what is sometimes called 'pure' psychology: it is something wider and deeper than either.[71] All psychology, on Husserl's view,—and psychology includes for him the Stumpfian phenomenology,—presupposes the attitude of natural science; it is a science of fact, a psychophysics.[72] There is, on the other hand, a science of 'pure' consciousness in the sense of consciousness freed from bodily entanglement and naturalistic presupposition:[73] a science that has to do, not with fact, but with 'essence' (*Wesen*).[74] This science, with its method of 'immanent inspection' or 'contemplation of essence' is phenomenology.[75] To enter upon it, we exchange the naive and

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[69]This statement must be judged in its context; were my intended book a history of contemporary psychology there would be much more to say. In a certain sense, for instance, Lipps is the direct antithesis of Brentano. At first, under the influence of Hume and Herbart, Lipps represented that 'psychologism' which Husserl attacks in the first part of his *Logische Untersuchungen*. Later he became a 'logicist,' but a logicist of the dialectic stripe, connected through Herbart and Fichte with Plato. Brentano was never anything but Aristotelian.—Meinong, Stumpf and Husserl are all directly related to Brentano. But they, too, have made their changes. It is a far cry from the Husserl of the *Philosophie der Arithmetik* (1891) to the Husserl of the *reine Logik*.

[70]E. Husserl, "Ideen zu einer reinen Phänomenologie und phänomenologischen Philosophie," *Jahrbuch f. Philos. und phänomen. Forschung*, i., 1913, 184: phenomenology is "von aller Psychologie durch Abgründe getrennt." Ten years before, the "rein deskriptive Analyse der Denkerlebnisse" which in psychology should precede explanatory or genetic endeavor is identified with phenomenological analysis ("Bericht über deutsche Schriften zur Logik," *Arch. f. system. Philos.*, ix., 1903, 114); and even in the *Ideen* (143, 159) a bridge is thrown across the abyss in the shape of an *eidetische Psychologie*. Messer ("Husserl's Phänomenologie in ihrem Verhältnis zur Psychologie," *Arch. f. d. ges. Psychol.*, xxii., 1912, 117 ff.; xxxii., 1914, 52 ff.) has done his best to placate the implacable.

[71]E. Husserl, "Philosophie als strenge Wissenschaft," *Logos*, i., 1910, 315; *Ideen*, 5, 121, 290.

[72]*Bericht*, 398, 400, 524 f.; *Philosophie*, 298 f., 302, 315; *Ideen*, 3 f., 8, 69 f. cf. O. Külpe, *Vorlesungen über Psychologie*, 1920, 22.

[73]*Philosophie*, 302, 315; *Ideen*, 57 ff., 94, 121 f.

[74]*Log. Untersuchungen*, ii., 18 f; *Philosophie*, 314 ff.; *Ideen*, 4, 7 ff., 114.

[75]"Immanentes Schauen:" *Philosophie*, 303, 313; "Wesenserschauung:" *Ideen*, 11, 43; "Wesensschauung:" *Philosophie*, 315 f. Cf. *Ideen*, 113.

dogmatic attitude of every day life and of natural science for a 'philosophical' attitude, which leaves visible only 'pure' consciousness in its 'absolute intrinsicality.'[76] And if we should rashly venture to transfer to the domain of descriptive psychology some result of the phenomenological scrutiny of essence, the responsibility is ours alone; Husserl washes his hands of us.[77]

Psychology, then, is the empirical science of mental facts as physics is the empirical science of material facts.[78] Psychology is concerned with 'experiences,' physical science with the 'non-experiences' to which experiences refer, with the 'intended objects' of acts.[79] And since natural science recognizes the individuation of organic life, these experiences are the experiences of an 'I.'[80] Consciousness, in the wide sense, therefore embraces the entire phenomenological make-up of the mental 'I', or consciousness is the phenomenological 'I' as 'bundle' or complication of psychical experiences.[81] In a narrower and 'pregnant' sense, consciousness is the inclusive name for intentional experiences or acts.[82]

The 'act,' it will be noted, is here identified with the complete intentional experience,[83] which includes both the 'content' (upon which the 'object' is based) and the 'intention' or 'act-character.'[84] Since, however, contents (in this narrower sense) are

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[76] *Philosophie*, 302, 315; *Ideen*, 3, 46 ff., 48 ff., 94, 120 ff., 182 f.

[77] The thing can be done (*Bericht*, 400; *Philosophie*, 315; *Ideen*, 143), and on Husserl's own showing the psychologist has no choice but to make the attempt. It seems, however, that every psychologist who has so far ventured (even the well-intentioned Messer) has flatly failed. Happily for us, such failure does not greatly matter. We are interested in Husserl, less for his own sake, than for the way in which psychologists have understood him.

[78] *Bericht*, 398.

[79] *Log. Untersuchungen*, ii., 338 f.

[80] *Ibid.*, 336; *Bericht*, 399 f., 524 f.; *Philosophie*, 298, 312 f.; *Ideen*, 104.

[81] *Log. Untersuchungen*, ii., 325 ff., 350, 354 f.; *Ideen*, 65, 168 ff. (esp. 172). Stumpf's phenomenology, which we have accounted a part of Husserl's psychology, has its phenomenological counterpart in a phenomenological hyletics; and this, directly translated into psychological terms, becomes a chapter of eidetic psychology (*Ideen*, 178 f.). But Stumpf is an interactionist, and would hardly rule out psychophysics ("Eröffnungsrede," *Dritter internat. Congress f. Psychol.*, 1897, 7 ff.).

Husserl's emphasis upon inference (*Log. Unt.*, 331, 339) has led Wundt ("Psychologismus und Logizismus," *Kleine Schriften*, i., 1910, 570 ff.) to criticize him from the side of the 'unconscious.' For our immediate purpose this criticism is irrelevant.

[82] *Log. Unt.*, ii., 342, 345, 349; *Ideen*, 168, 174 f. Husserl's terminology has changed in the *Ideen*: for 'act' see *ib.*, 170.

[83] So, *e. g.*, *Log. Unt.*, ii., 323, 357, 362, 388; cf. A. Messer, *Empfindung und Denken*, 1908, 43.

[84] For the "primary contents" see *Log. Unt.*, ii., 652 (cf. 330, 345, 349, 360, 364 *n.*, 370 ff., 468, 471); *Ideen*, 172. For the 'basing' of the object, *Log. Unt.*, ii., 353, 361, 362, 363, 370, 393, etc. For 'intention,' *ibid.*, 323, 348, 357 f., 361, etc.

themselves non-intentional experiences, we may use the term 'act' in contradistinction to content for the act-character alone.[85] Acts, in this specialized meaning, lack intensity, but show differences of quality and material. These moments, though inseparable, are independently variable.[86] Quality is that which marks an act as an act of ideation or judgment or question or doubt or wish.[87] Material is the specific direction of an act upon its object. Thus I may apprehend a given geometrical figure now as an equilateral and now as an equiangular triangle. Here the objects are the same; the contents are the same; the act-qualities are the same; but the act-materials are different. That is to say, the material of an act determines not only what object is apprehended, but also as what (with what attributes, forms, relations) the apprehended object is taken.[88]

Within this analytical framework Husserl seeks specifically to test the validity of Brentano's law,—the law to which we have found Münsterberg raising formal objection: namely, that all psychical phenomena either are ideations or rest upon ideations as their basis.[89] Husserl is able to show that Brentano's formula involves an equivocation. Translated provisionally into his own terms the law would run: every intentional experience either is an ideation (*i. e.*, a bare or simple ideation) or has an ideation as its basis; but here the 'ideation' of the first clause means an act-quality, and the 'ideation' of the second clause an act-material.[90] Husserl accordingly enters upon an elaborate analysis of the term 'ideation,' which he equates, in its very widest sense, with the term 'act of objectification.'[91] The new genus may be differentiated, qualitatively, into thetical and athetical acts of objectification: the former being the acts of 'belief' in the sense of J. S. Mill or of 'judgment' in the sense of Brentano, and the latter being the corresponding acts of 'simple ideation;' and, materially, into propositional and nominal acts ("Columbus discovered Amer-

[85]Husserl himself speaks of the intentional content (*Log. Unt.*, ii., 375, 378, 386 ff.) and of the intentional essence of the act (392 ff.). It is, however, hardly possible, in any extended discussion, to avoid the narrower use of the term: cf. Messer, *Empfindung und Denken*, 45, 47, 74.

[86]*Log. Unt.*, ii., 374, 386 ff., 391. Cf. 566.

[87]*Ibid.*, 386, f.

[88]*Ibid.*, 389 f.; for a broader definition, cf. 462. In the *Ideen* the terminology has again changed: see esp. 267 f. Here and in *Bericht*, 244 the distinction of quality and material is ascribed to Brentano: I suppose the reference is to the distinction of quality (affirmation, negation) and *Sinn* in Brentano's doctrine of the judgment (*PES*, 283, 303).

[89]See above, note 15.

[90]*Log. Unt.*, ii., 428 f.

[91]*Ibid.*, 447: cf. 449, 458 ff.

ica," "Columbus, the discoverer of America").[92] Brentano's law may now be rewritten in the form: every intentional experience either is an act of objectification or has such an act as its basis; so phrased, the formula is valid.[93] The important thing for Husserl is, no doubt, that he has thus thought himself clear. The important things for us are that, by keeping his discussion at the phenomenological level, he has avoided all reference to the attitude of an 'I', whether empirical or pure,[94] and that with 'act of objectification' he has introduced a term which seems destined to play a large part in empirical psychology.[95]

§11. Husserl's influence may, indeed, be traced all through the later and more characteristic work of the Würzburg school. How deeply it had affected the psychology of Külpe himself, we shall probably never know. We have, however, a *Psychologie* from the hand of Messer, a member of the school, whose thinking has been largely shaped by Erdmann, Husserl and Külpe.[96] This book, in default of the promised recasting of Külpe's *Grundriss*, must now engage our attention.

Messer offers three characterizations of the conscious or psychical. He accepts from Münsterberg the formula that physical is shareable, psychical unshareable experience; he accepts from Lipps the view that the psychical always is, while the physical is not, in some sense 'mine;' and he accepts from Husserl the distinction of the psychical as immanent from the

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[92] Ibid., 440 f. In the *Ideen* (235) the terms 'thetical' and 'athetical' are replaced by the broader terms 'positional' and 'neutral', with consequences that do not immediately concern us.

[93] *Log. Unt.*, ii., 458. The second clause may also be paraphrased: "or necessarily includes as constituent an act of objectification whose total material is at the same time and in the sense of individual identity—*its* total material."

[94] In the *Log. Unt.* (340 ff.) the 'pure I' is phenomenologically discredited. In the *Ideen* (109 f.) it comes back, but the consequences again do not immediately concern us. Only, the cutting of Münsterberg's difficulty, in the *Log. Unt.*, would appear to have been premature.

[95] As in Dürr's edition of Ebbinghaus' *Psychologie*, 1911-13.

[96] Messer published in 1908 a little book entitled *Empfindung und Denken*, which bears Husserl's impress on nearly every page. Its main effect upon the reader's mind is a sheer wonder that two things so incompatible as sensation and thinking can lie down together between the same covers. The *Psychologie* of 1914 has the more empirical flavor of the Würzburg school.

Külpe's posthumous work, *Vorlesungen über Psychologie* (ed. K. Bühler, 1920) is utterly inadequate on the side of its author's system. It does not either show Külpe at his best; much of the writing is the work of a jaded and driven man. In any case, Külpe may have been less nearly ready than we supposed. Bühler's preface ends with the (to me surprising) statement: "Über den Willen und das Denken hat Külpe nicht gelesen und leider auch keinerlei Aufzeichnungen hinterlassen."

physical as transcendent.[97] We may pass over Münsterberg and Lipps,[98] and come at once to Messer's interpretation of Husserl. Since the transcendence which characterizes the physical is a transcendence of consciousness, it follows of necessity that some psychical must, for Messer, take the form of 'consciousness of.' As a matter of fact, Messer declares roundly that all consciousness is consciousness-of, *Gegenstandsbewusstsein*, though certain elements of consciousness, taken in isolation from their regular setting, lack intentionality.[99]

The experiences (*Erlebnisse*), which make up the subject-matter of psychology may be divided into those of knowing, feeling and willing.[100] We may also speak of consciousness of objects (in a narrower sense), consciousness of state, and consciousness of cause.[101] The elements of these experiences are classified as palpable or impalpable, according as they stand up

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[97] A. Messer, *Psychologie*, 27 f., 32 f.; cf. 55, 73, 127 f., 138, 146. Messer is here speaking of *das bewusst Psychische*. Whether there are also *unbewusst psychische Vorgänge* is a question that he leaves open, though he inclines to answer it affirmatively: 35 ff., 251 ff., 365 f.

[98] For Münsterberg's position, see *Grundzüge*, i., 1900, 72. The Lippsian 'mine' does not mean for Messer the constant and overt presence in consciousness of 'my I itself;' the I-character or relation of 'mine' is often represented solely by the unitariness and blendedness of experiences: *Psychol.*, 27.

[99] *Ibid.*, 66, 53. The sweeping statement, as always, brings its difficulties. Messer admits, *e. g.*, that conscious complexes (fusions of pleasant feeling with sexual sensations, fusions of unpleasant feeling with sensations of pain) may occur without reference to an object (307). Such complexes are obviously far removed from the status of conscious elements; and pain, at any rate, may be so overwhelmingly itself as to drive away all competitors of the referential kind.

There is difficulty, moreover, in connection with Messer's whole doctrine of emotion (*Affekt*). In *Psychol.*, 52 feelings and emotions (apparently, all feelings and all emotions) are intentional. But the simple feelings of pleasantness and unpleasantness are positively not intentional (302), and emotions are only strong and sudden feelings (293). I find, indeed, no refference to affective intentionality in the pages that deal in detail with emotion and its classification. A feeling may, however, carry intentional reference in its own right,—in which case it is no longer a bare pleasantness or unpleasantness, but a *Wertgefühl* or affective evaluation (303). Hence in a later list we find no mention of "feelings, emotions," but only of "experiences of evaluation and will," as intrinsically intentional (374; cf. 52). In general it seems that Messer recognizes three levels of emotive process: (1) a fusion of feeling with sensations, wholly without objective reference; (2) a fusion of feeling with acts of the consciousness of objects (in the narrower sense), *i. e.*, a complex in which the feeling is not intentional but the basal ideation carries objective reference; and (3) a fusion of affective evaluation with ideation, a complex in which both principal factors carry reference (cf. 66). How these types are to be fitted to his definition of consciousness is not easy to see.

[100] *Ibid.*, 65.

[101] *Ibid.*, 66. This classification is borrowed from J. Rehmke, *Lehrbuch der allgemeinen Psychologie*, 1894, 148 f.

under observation or as they refuse to be observed and must accordingly be recovered through reflection.[102] Of the palpable elements, sensations belong to all three types of consciousness.[103] Knowing, or the consciousness of objects, includes further, as palpable elements, the images which correspond with sensations, temporal and spatial contents, and the impressions (of 'same,' 'like,' 'different,' etc.) which lie at the basis of general concepts.[104] Whether consciousness of state and consciousness of cause embrace palpable elements of a specific (non-sensational) sort is difficult to say: Messer's statements are conflicting. It seems that the simple feelings are, as a class, impalpable, though in exceptional cases they will bear scrutiny.[105] Conation, too, while it is intentional and should therefore by rights be impalpable, may, on occasion—if it is aroused involuntarily, and especially if it is directed upon objects of sense-perception—be observed during its course.[106]

The term 'act' is used by Messer in two senses: first, for the whole of an intentional experience, and secondly for the act-side or act-character of such an experience.[107] In the first sense, acts are called palpable or impalpable according as their intended object is or is not represented by sensations and images.[108] In the second sense, in which the acts are conscious elements

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[102] *Ibid.*, 48, 74, 202; *Empfindung und Denken*, 78 f. I suggest 'palpable' and 'impalpable' as the English equivalents of *anschaulich* and *unanschaulich:* cf. 'Macbeth,' II, i., 40.

[103] *Psychol.*, 66, 74 f.

[104] Messer recognizes peripherally excited and centrally excited (or reproduced) sensations in primary and secondary (synaesthetic) form: *Psychol.*, 127 ff. For space and time as contents, see *ibid.*, 149, 155, 175 f., 202. It is expressly said that space is not an attribute of sensation, like quality and intensity (149); and time is so far from being an attribute of anything that it may, in Messer's opinion, be experienced for itself, as empty time (176). Yet we are told later that feelings share with sensations the attributes of intensity, quality, and the "extensives Merkmal" of temporal duration, while they lack the "räumliches Charakter" that attaches to all sensations (280)! For the palpable impressions underlying concepts, see *ibid.*, 180 ff.

[105] *Ibid.*, 48: feelings that are closely connected with sensations and that possess a 'peripheral' character are palpable. But sensations are defined differentially as palpable (74); and feelings are impalpable (278 f., 346). Husserl (*Log. Unt.*, ii., 369 ff.) and Stumpf recognize a class of affective sensations, and thus meet the difficulty. In *Empf. und Denken*, 23, Messer takes the same view: cf. Psychol., 276.

[106] *Ibid.*, 48. But all *Streben* is involuntary (312)! Moreover, the later distinction seems to be, not that of peripherally and centrally directed conations, but rather that of less definitely directed conation and more definitely directed desire (312). All conation 'aims at' something (311, 314). Husserl is ready to admit sensations of desire or sensations of impulse (*Begehrungsempfindungen, Triebempfindungen*) as non-intentional elements of will (*Log. Unt.*, ii., 373 f.; *Ideen*, 172).

[107] *Psychol.*, 53, 202.

[108] *Ibid.*, 139, 191, 296, 346.

abstracted from the whole of an intentional experience, they are always (with the exception of peripherally directed conation) impalpable.[109] Messer does not attempt to draw up a list; we must therefore make it up for ourselves as best we can.

Under the head of consciousness of object, we have, first, the acts of ideation in the wider sense: acts of perception, of memory and of imagination.[110] Then follow the acts of conception or thinking or knowing (*Wissen*).[111] Here we find, to begin with, the experience of concept or meaning.[112] Messer further names the acts of relating, of comparing, of judging, and of knowing in the pregnant sense (*Erkenntnis*).[113] Judgment is a synthetic act of relating, comprising at least two members, which is accompanied by the strictly elementary act of affirmation or negation.[114] Since every synthetic act may be translated into a simple act, the propositional act of judgment has a nominal act as its parallel.[115] Since, moreover, judgments may be passed with all degrees of subjective assurance, we have attendant acts ranging from conviction to conjecture.[116] Finally, over against judgment stand supposal, which bears the same relation to judgment as imagination to perception and memory, and the bare entertainment of a thought, shorn of all reference whether to validity or to invalidity.[117]

Later in the book we come upon the acts of intellectual evaluation and intellectual preference. The former, as judgments of value, may be subsumed to the general category of judgment. It is not clear that the acts of preference may be subsumed, in like manner, to the general category of comparison.[118]

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[109] *Ibid.*, 202 f. These acts are not characterized attributively, as they are by Husserl and by Messer himself in *Empf. u. Denken* (50 ff.). We get a hint of quality and material, however, in such passages as *Psychol.*, 138 f., 204, 208.

[110] *Ibid.*, 139, 191 f. Messer does not seem to be quite as certain as Husserl (*Log. Unt.*, ii., 364; *Ideen*, 224 ff.) of the specific act-character of imagination. He says, indeed,—though in the context of explanatory psychology,—that the distinction of memory and imagination springs rather from practical and epistemological than from psychological needs (*Psychol.*, 346).—It is to be noted that the total acts (intentional experiences) of memory and imagination may be either palpable or impalpable: 221, 346.

Whether the *Bildbewusstsein* of *Psychol.*, 138 is elementary, we are not told. According to *Empf. u. Denken*, 60 f., it is not.

[111] *Psychol.*, 139, 202.

[112] *Ibid.*, 207.

[113] *E. g.*, *ibid.*, 214, 212, 209, 216.

[114] *Ibid.*, 207, 211, 212 f.

[115] *Ibid.*, 208.

[116] *Ibid.*, 219.

[117] *Ibid.*, 220.

[118] *Ibid.*, 303 f., 305. In the latter passage, *Vergleichung* and *Vorziehen* are distinguished.

Under the head of consciousness of state, we have as impalpable elements, first, the non-intentional simple feelings (or the great majority of them) and secondly the objectively directed feelings (affective evaluations, feelings of value) and the corresponding acts of affective preference.[119] Lastly, the consciousness of cause includes conations, or at least those definitely directed conations which merit the name of appetition or desire, and acts of will. Messer insists that conations and acts of will belong to distinct classes of elementary experiences.[120]

There remains the phenomenon of attention. Descriptively regarded, attention is not an act: it is rather that attitude (*Verhalten*) of the I in which our consciousness of objects (the phrase is used, at first, in the narrower sense) is formed or constituted: "objects exist for us only in so far as we are attentive to them."[121] Attention thus stands in intimate relation to the consciousness of objects; indeed, we need not scruple to use this phrase in its wider sense, seeing that the objects of affective evaluation, of affective preference and of will are also objects of attention.[122] While, however, the mere fact that an object is given us guarantees the presence of attention, the increase of the clearness and distinctness of the object with increasing concentration of attention is sufficiently regular to serve as a descriptive character.[123]

§12. The systems which, in their phenomenological or descriptive aspects, we have now briefly reviewed may fairly be considered as typical of the whole psychology of act. Our purpose is critically to survey this psychology, and in particular to decide whether it has been more successful than functional psychology in its attempt to establish a special class of 'psychical' phenomena as the given subject-matter of psychological investigation. There are, however, among the authors to whom we have referred, two—Witasek and Messer—who, as exper-

[119] *Ibid.*, 276, 303, 305. For emotions, see note 99 above.

[120] *Ibid.*, 311.

[121] *Ibid.*, 254.

[122] *Ibid.*, 256.

[123] *Ibid.*, 256, 267; cf. 50 f. Messer seems to have forgotten that the object is ordinarily transcendent, so that its clearness and distinctness cannot serve as a psychological character of attention (137 f.). It is true that his instance, of sensitivity and sensible discrimination, implies theoretically an immanent object; but he has told us (140 f.), in regard to this very matter, that in point of fact the observer usually adopts the 'natural,' 'objective' attitude. Surely, then, it is clearness and distinctness of the 'content' or 'sense' of the act (the material, in Husserl's wider sense) that must characterise the attentive experience. See, however, *Empf. u. Denken*, 120, note 3.

imentalists, make appeal to our own interest, and who, as writers of text-books, seem directly to challenge comparison. Witasek, as we have said, belongs to the school of Meinong, and Messer to the schools of Husserl and Külpe. Let us see, then, as a preliminary to our main task, in how far these psychologists agree in their teaching.

The question that naturally stands first, the question of the classification of psychical phenomena, we shall discuss later. Passing this by, we take up in their order the principal points of the two systems.

(1) Both Witasek and Messer recognize the distinction of act and content. But Witasek regards these moments as inseparable; there is no act without content, and no content without act; whereas Messer affirms that acts (the act-characters of intentional experiences) may stand alone, as fully constitutive of consciousness, and that sensory contents may appear in the background of consciousness unaccompanied by acts.[134]

(2) Witasek accepts Brentano's law without reservation; there is no judgment, feeling or desire that is not based upon ideation. Messer, on the contrary, regards all consciousness as consciousness-of: a pleasant or unpleasant feeling may, by its intrinsic nature, be objectively directed; and an object "may just as originally be desired or willed as ideated and thought." Yet it cannot be said that Messer rejects the law: his statements are always qualified.[135]

(3) Witasek defines sensations as "perceptive ideations of the simplest possible contents." There is, therefore, an act of sensation in addition to the sensory content. For Messer there is no act of sensation; the sensory

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[134]Witasek, *Grundlinien d. Psychol.* (cited henceforth in this § as *W*), 1908, 75; Messer, *Psychol.* (cited henceforth in this § as *M*), 203, 255. In *Empf. u. Denken*, 1908, Messer grants that sensations may appear, without acts, in the background of consciousness (40), but leaves the separate occurrence of contentless acts an open question (100 ff.). Husserl, in *Log. Unt.*, ii., seems to accept the actless content (372, 427), but denies that the act-character, the complex of quality and material, can stand absolutely alone (560 ff.; cf. 68 ff.). In *Ideen* (172) he leaves both questions unanswered. Lipps (*Leitfaden d. Psychol.*, 1909, 15) asserts that all contents or images are, implicitly or explicitly, representative, images of objects; whether there is a strictly "imageless thinking" he will not decide. Stumpf believes that sensations may be present and may undergo change without our 'remarking' the fact: but then these sensations are phenomena, not subject-matter of psychology. He inclines toward the acceptance of imageless thinking: but, again, every function must on his view have some sort of correlated content (*Erscheinungen*, 1907, 11, 25, 34).—The experimental data regarding imageless thought do not here concern us.

[135]*W*, 97, 315; *M*, 66, 303, 314. In the first passage from *M*, feeling and will "somehow include or presuppose consciousness of objects [in which sense?];" it is as if Messer had not yet contemplated the chapter on value. In 303 there is an 'intimate connection' of knowing and feeling: but is the knowing basal? In 314 the reference of conation and willing to objects may be termed "practical ideation," ideation being taken "in the most general meaning of the word." Yet the objective reference has just been declared intrinsic!—In *Empf. u. Denken*, 53 ff., Messer accepts Brentano's law in Husserl's formulation.

[illegible]

[illegible] Erst. u. Denken, 24 ff. [illegible] For Lipps the [illegible] are the [illegible] new products [illegible] : Leitfaden, [illegible]

[illegible] W., 225, 232; M. 150; Erst. u. Denken, 25 f. Stumpf, Erch., 4, 7, 16, 22 f., 33, has a class of relations distinct both from phenomena and from functions. Lipps ascribes the relations to apperception: they are "modes in which objects are referred to one another in the apperceiving I," though they are "unequivocally determined by the objects" themselves: Leitfaden, 1909, 161, 164.

imagination and of memory: the latter (as we shall see) belongs to another part of Witasek's system.[129]

(7) Both Messer and Witasek recognize the specificity of judgment. We note, however, several points of difference. (*a*) For Messer, the act of judgment is always at least bimembral; the act is, in Meinong's phrase, synthetical. For Witasek, judgment may be either synthetical or merely thetical. (*b*) Witasek finds in the act of judgment two invariable moments, affirmation-negation and belief or conviction, and an occasional moment, evidence (of certainty, of probability). Messer identifies the invariable moments: affirmation-negation, acknowledgment-rejection, taking-as-true (as-untrue) and conviction are, for him, one and the same. Evidence he regards as a condition of affirmation-negation, and judgments experienced evidentially for the first time he marks off as acts of knowledge (*Erkenntnis*). (*c*) Witasek and Messer agree that a judgment may be passed with different degrees of subjective assurance. For Witasek, however, the difference resides in an intensive variation of the act-moment of conviction; for Messer, who does not recognize intensive gradation of act, it consists in the replacement of a taking-as-true by a taking-as-probable or a taking-as-possible. Judgments of possibility, in Witasek's system, are judgments of subsistence, as distinguished from judgments of existence. (*d*) Messer subsumes inference to judgment: it is judgment whose relational members are themselves judgment-contents. Witasek looks upon inference as common both to judgment and to supposal.[130]

(8) Messer's perception, as intentional experience, is practically identical with Witasek's produced ideation. Perception proper (as distinct from perceptive ideation) is for Witasek a special case of judgment. Messer, as we have seen, makes all judgment at least bimembral.[131]

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[129] *W*, 246 ff.; *M*, 192 f. Here the ideas of imagination and recollection are the two principal species of *Vorstellungen*, and recollection is distinguished from the mere 'renewal' of a perception. Yet *M*, 221 hints at an act-difference between perception as such and ideation as such (cf. K. Koffka, *Zur Analyse d. Vorstellungen u. ihrer Gesetze*, 1912, 270 ff.). In general, *M* speaks only of imagination and recollection.—Stumpf (*Ersch.*, 16) includes *Empfinden* and *Vorstellen* under *Wahrnehmen:* it seems that only the phenomena differ. Lipps has a bare *Vorstellen* as receptive experience or *Widerfahrnis*; he has also acts of imagination; and he has acts of (introspective) recollection both at the level of perception and at that of judgment (*Leitfaden*, 1909, 16 ff., 20 f. 336). For Husserl's analysis see *Log. Unt.*, ii., 463 ff., 471 f.

[130] *W*, 279 ff., 295 f., 310; *M*, 206 ff.; *Empf. u. Denken*, 138 ff. The doctrine of judgment (like that of form-quality, which I avoided in a previous note) is too detailed for discussion at this point. For Lipps' view, that judgment is my acknowledgment of an object's claim, see G. Anschütz, "Theodor Lipps' neuere Urteilslehre: eine Darstellung," *Arch. f. d. ges. Psychol.*, xxx., 1914, 240 ff., 329 ff. I further note only that Stumpf (*Ersch.*, 26; cf. Brentano, *PES*, 260 ff.) finds in judgment "a new functional attitude;" that Messer's distinction of one-rayed and many-rayed acts (*M*, 207) derives from Husserl (*Ideen*, 247 f.); and that Meinong discusses thetical and synthetical judgments in *Ueber Annahmen*, 1902, esp. 145.

[131] *W*, 239, 288 ff.; *M*, 162. Stumpf, following Brentano (*Tonpsychol.*, i., 1883, 96; cf. *PES*, 277), at first raised perception to the rank of judgment; he now (*Ersch.*, 16) makes perceiving prejudgmental. Lipps uses *Wahrnehmung* in two senses: for a *Widerfahrnis*, and for the consciousness of reality of the presented object. In either case perception is prejudgmental (*Leitfaden*, 1909, 15 f; Anschütz, *op. cit.*, 334).

(9) Recollection and recognition are also, for Witasek, forms of judgment. Messer does not distinguish between them; and his act of recollection or act of recognition is not a judgment, but a mode of ideation.[illegible]

(10) Both Messer and Witasek, again, recognise the specificity of supposal. According to Witasek, however, supposal stands to judgment as reproduced ideation (ideas of memory and imagination) stands to perceptive (produced) ideation; according to Messer, it stands to judgment as imagination stands to perception and memory (recollection). Moreover, Witasek's supposal includes what Messer distinguishes as supposal and as the bare entertainment of thoughts. And Witasek's supposal further covers hypothesis, which is for Messer a "more or less probable judgment."[illegible]

(11) Attention receives markedly different treatment in the two systems. Messer, we remember, places attention outside of his three phenomenological classes. It is neither knowing nor feeling nor willing; it is an attitude of the subject wherein and whereby objects are constituted. Witasek finds the nuclear fact of attention in a thetical act of judgment, an act of apprehension. Attention is thus on all fours with perception, recollection and recognition.[illegible]

(12) In the psychology of feeling we expect differences. It is noteworthy, nevertheless, that for Witasek feeling is an act, while for Messer it is [illegible] in the case of feelings of [illegible] content. It is curious, too, that the distinction between sense-feeling and aesthetic feeling is drawn on

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[illegible] Lipps has a feeling of familiarity [illegible] the acts "perceptive" and judgmental of recollection: [illegible]

[illegible] Lipps agrees with Messer as regards hypothesis [illegible] supposal in general to be subjectively conditioned [illegible] Stumpf also hesitates to accept the [illegible] Husserl looks upon supposal as a [illegible] *Modification der* [illegible] and declares that Meinong's *Annahme* is an equivocal term [illegible] For Meinong himself, supposal is a fundamental [illegible] he includes it along with judgment under the general heading of thought [illegible] If one is bent upon making it a form of judgment, it will be a judgment of imagination [illegible] *Ueber Annahmen*, [illegible]

[illegible] attention is not constitutive of our consciousness of objects, but is simply "an especially high degree of the consciousness of objects" [illegible] the whole range [illegible] and he is clear that [illegible] must be there if we are to [illegible] attend [illegible] whether this attending is [illegible] It is rather a [illegible] issuing from the [illegible] in the object; it is [illegible] Messer in M [illegible]

diametrically opposite lines. According to Witasek, the aesthetic feelings are directed wholly upon ideational content: change of act (as from sensation to reproduction) leaves them unaffected. Sense-feelings, on the other hand, are essentially bound up with ideational act: pain felt and pain remembered are radically different things. According to Messer, the sense-feelings are feelings which attach directly to sensory contents, and the aesthetic feelings are feelings based upon "acts of the consciousness of objects." The difference which we expected could hardly be more extreme.[135]

(13) Messer devotes a chapter of his book to the consciousness of value. The primitive form of this consciousness is the affective, which at its simplest is an act of feeling, *i. e.*, an objectively directed pleasantness or unpleasantness. In Witasek's system its position is very different. Just as there are feelings based upon acts and other feelings based upon contents of sensation, so it is with judgment: the logical feelings or feelings of knowledge are based upon acts of judgment, the feelings of value or ethical feelings upon judgment contents. The difficulty which we feel in Messer's account—how a simple feeling, any more than a simple sensation, can of itself assume or acquire intentionality—is thus avoided. Moreover, Witasek is able to proceed from judgment to supposal. There is no feeling based on the act of supposal; but there are feelings—play-feelings, in contradistinction to real or serious feelings—based upon its contents. Of these Messer says nothing.[136]

(14) In the psychology of volition, too, we expect differences; and again, we are not disappointed. The act of will which for Messer is elementary, and which is to be distinguished from the equally elementary conation or desire, is for Witasek the highest development of that same elementary desire. According to Messer, an object may be desired or willed as directly as it may be ideated or thought; according to Witasek no object, but only an objective, may be desired or willed; desire rests always upon some supposal. And so the differences continue. It may be remarked, as a curiosity of system-making, that in Witasek's chapter the simple reaction figures, in Külpe's sense, as the primary means "of an exact experimental investigation of volition, indeed, of conation at large," while Messer, a member of Külpe's school, disposes of it in his chapter on attention.[137]

These fourteen points may suffice to show the likenesses and differences of the two systems. There is resemblance. We saw that there was a likeness between Wundt and Brentano as long ago as 1874,[138] and it would be strange if there were none between two experimental systems of forty years later. The resemblance, too, is more than general; it is a family likeness; the systems are of the same type. Yet the differences are many, so many that every chapter invites us to a choice between alternatives.

To trace the sources of such difference is not an easy matter. It is plain on the surface that Messer's system is syncretistic

[135] *W*, 324 f.; *M*, 295 f.

[136] *W*, 328, 330 f.; *M*, 303. For Lipps and Stumpf, feelings of value always imply judgments (*Leitfaden*, 1909, 341 f.; *Ersch.*, 27, 30). Husserl sets the problem in his own terms in *Ideen*, 239 ff.

[137] *W*, 349, 351; *M*, 311, 314: for reaction, *W*, 363, *M*, 265, 273. Lipps derives will from conation (*Leitfaden* 1909, 258, 301 ff.). Stumpf (*Ersch.*, 26 f., 30) seems to include will under the emotive functions.

[138] Cf. this JOURNAL, xxxii., 1921, 110 f.

[illegible]

[illegible]

The notion of functional indefiniteness [illegible] is, in [illegible]'s own showing, altogether out of place in the context of [illegible] the distinction of descriptive and functional concepts in "Ueber den Begriff des Aktes," Arch. f. d. ges. Psych., xxiv, 1912, 250 ff. For the notion itself, see G. E. Müller, Zur Analyse d. Gedächtnistätigkeit u. d. Vorstellungsverlaufes, iii, 1913, 565 ff.

[illegible]'s enthusiasm leads him to remark that "the transcending which is peculiar to our mind, the direction upon objects, is in strictness a function only of judgment or supposal" (p. 310). At least an incautious statement! For if direction-upon is the ear-mark of the psychical, and if the direction of an ideation is due to the "cooperation" of a judgment or a supposal, it follows that the ideation as such is not psychical: in which case [illegible]'s law implies the commingling of psychical and non-psychical at the very center of the psychical realm.

starting-point of intentionalism our authors have taken widely divergent paths. The question remains, however, how deep the sources of divergence lie,—whether they are only superficial and accidental, or whether they are fundamental. Until this question is faced and answered, we cannot either affirm or deny that psychology may be wrought out in terms of a peculiar class of intentional facts.

We shall put the act-systems, first, to a triple test, by considering their attitude in regard to classification, and to the special topics of sensation and attention.

(1) We begin with the classification of psychical phenomena. Here, again, it would be easy to show that the systems differ. Indeed, they differ so radically that one, two, three or four ultimates may be recognized.[141] We must remember, however, that classification is, primarily, a matter of convenience, and that the functional and experimental schools have also been unable to supply a classification that should be generally accepted. All that the differences prove, therefore, is that intentionalism is no unerring or unequivocal guide to arrangement. That is worth noting: but we shall get more light if we consider the classifications adopted by an individual psychologist at different stages of his systematic thinking. We have, fortunately, two examples of the kind required, in the works of Stout and Lipps.

The classification put forward in the first edition of Stout's *Manual* (1899) is very simple. It may be represented as follows:

I. Ultimate modes of being conscious of an object
 - *a.* The cognitive attitude or cognition or knowing
 - *b.* The feeling attitude or feeling [always dependent upon cognition]
 - *c.* The conative attitude or striving [coordinate with cognition]

II. Experience not at the moment contributing to the cognitive function of consciousness
 - *d.* Sentience or sub-consciousness

There can be no doubt that Stout is here trying to cover the whole field of consciousness by way of objective reference. Sentience, the outlying category, is after all nothing more than cognition at rest: modifications of consciousness that may and

[141]The single ultimate, of which we have so far had no example, is characteristic of the conational system of S. Alexander: "there is but one ultimate mental process [a continuous tissue of acts, or awarenesses, or enjoyments], namely conation" ("Foundations and Sketch-plan of a Conational Psychology," *Brit. Journ. Psych.*, iv., 1911, 243; cf. H. A. Reyburn, "Mental Process," *Mind*, N. S. xxviii., 1919, 19 ff.). Something of the same sort appears in the psychology of P. Natorp. Consciousness has three moments, which may be distinguished by abstraction: the I, the content, and the relation between them. Since the I is presupposed by psychology, and since the relation to the I is an irreducible and indescribable ultimate which, like the I, is a precondition of psychology, it follows that psychology has to do only with content (*Allgemeine Psychologie*, i., 1912, 24, 33.).

[illegible] time, not to be dis- [illegible] and must accordingly [illegible] They are, never- [illegible] arrests, only at a lower [illegible] in presentation.[142] [illegible] is less simple. [illegible]

[illegible] are them- [illegible]

[illegible] or ultimate [illegible] objects [illegible]

[illegible] we cure at once [illegible] Stout has [illegible] reference [illegible] of 1913. [illegible] sentence is [illegible] was presenta- [illegible] moment hence. [illegible] from its al- [illegible] into a new [illegible] distinct from [illegible] subjective, finds [illegible] differentiation of [illegible] system has [illegible]

[illegible] is also very simple. [illegible] conscious contents or [illegible] subject-matter of psy- [illegible]

[illegible] That all was [illegible] thought-processes, [illegible]

[illegible] three forms of simple [illegible] and attention is conation [illegible]

I. "The directly experienced I with its determinations, the feelings" (absolutely subjective contents);

II. "The contents of sensation and sense-perception, *i. e.*, the simple sensory contents, the complexes of sensory contents, and the spatial and temporal forms and modes of their arrangement" (absolutely objective contents);

III. "The directly experienced relations of the I to what is objective, and the relations of the I in general" (intermediate contents); and

IV. "The ideational contents corresponding with all these conscious contents" (secondarily objective contents).

This table is exhaustive; it names all the genera of contents of consciousness; "there are no other conscious experiences."[145] And when the reader has assured himself that the "phenomenal acts" belong to the class of relations, or contents intermediate between the I and its objects, the complete outline of Lipps' system lies before him.

In 1909, however, Lipps has given up the idea of an inventory of consciousness. Psychology now has to do with "consciousness and conscious experiences;" and as this subject-matter unfolds, in the introductory sections of the book,[146] we find an intercrossing complexity that cannot by any trick of strait-jacketing be reduced to a single table. The following summary shows some of the complications with which the beginning student must contend.

I. Lipps speaks of experiences, contents, acts, activities, states and colorings of consciousness.

Contents are not experiences; they are rather the images or impressions experienced in consciousness.

Acts, too, are not intrinsically experiences. In our acts of thought and conation we do not ordinarily experience the objects apprehended and desired. We experience objects "only in so far as we have adequate images of them."

Activities and states (which latter are identical with colorings) are experiences.

II. Experiences are either objective or subjective. The type of objective experience is sensation, the having of a simple sensory content. The term covers also experiences of bare sense-perception, the mere having of a complex of sensory contents in spatial and temporal arrangement; and the bare ideation in which we have, as imaginal content, the image of some object in the outside world. All other experiences are subjective. Their type is the feeling.

The acts of thought, again, although (as we have seen) they are not intrinsically experiences, are either subjective or objective, according to the nature of their object.

III. It is to be noted that 'experience' by no means implies completeness or independence of the conscious datum so named. In sensation, for example, I have as objective experience the having of a sensory content, and as subjective experience the experience of myself as sensing. Every objective

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[145] Lipps, *Leitfaden*, 1903, 16 ff., esp. 20.

[146] *Ibid.*, 1909, 1-43.

experience thus includes or incorporates a subjective experience. In the same way my acts, though not in themselves experiences, are subjective experiences in the sense that in and with them I experience myself as thinking, desiring, etc.

IV. Lipps distinguishes receptive experiences, acts and states. The receptive experiences are those that we 'run up against:' sensations, the experience of claim. At the opposite pole from them stand the acts of conation, in which we aim at some object. The affective states differ from both.

V. Sensations are differentiated as sight, hearing, etc.

States are all included within the opposition pleasantness-unpleasantness, or move in that dimension. Since, however, many states are named, it appears that a further differentiation must be made.

Acts are specified in some detail. Thus we have acts of production (evocation of images of imagination), acts of mixed reception and produc-duction (thought), acts of aiming (conation), acts of 'bracing up to' and 'putting the final touch on' (starting and stopping points of conative activity), acts of acknowledgment (judgment). The relations are not quite clear. Perhaps there are two genera of acts: the one including as species the acts of imagination, thought and judgment, the other those of conation and conative activity.

VI. Conation and activity may be either active or passive.

Conation and activity may, further, be either inner or outer, according to the nature of the object on which they are directed. Inner activity is activity of apprehension, activity of imagination, activity of apperception (which latter is, again, variously specified). Outer activity is bodily activity.

It appears that we have in the experiences of conation (subjectified experience of claim) and of conative activity (feelings of actual and potential activity) a foundation or undercurrent of subjective experience distinguishable from the specific feelings which colour it: but the point is not clear.

VII. Along with acts and activities are given subjective experiences of their relation: experiences of conditioning, of issuance, of dependence, which Lipps groups together as experiences of motivation.

VIII. Every conscious experience and everything experienced in consciousness may later recur in the form of a reproductive image or image of ideation.

IX. Consciousness is intentional, but it is intentional in varying manner and degree. Sensations contain an object only potentially or implicitly. Acts of thought explicate these objects, in such wise that consciousness may thereafter busy itself with them; there is in thought "a peculiar interaction between the I and the objects." Conation and conative activity are, on the other hand, always 'aimed at' something; they 'are' the interrelation or the cooperation between the object with its claim and the individual consciousness. And since all feelings are colourings of this conative activity, I cannot feel without feeling myself somehow related to an object; I am cheerful or depressed, confident or in despair, 'about' something. Finally the experiences of motivation appear to be intentional in the same way as experiences of conation in general.

There is little here to suggest the tabular statement of 1903. No doubt, all the four classes of the earlier edition may be traced in the later; but our efforts at precise arrangement are baffled, and we wonder whether, after all, the simplicity of Lipps' first exposition was not itself rather apparent than real. At any rate, no clue to the psychological labyrinth is now to be found among

the conscious experiences. If we wish to set things in order we must go below and behind consciousness to the unconscious real.—

Summaries are tedious to make and tedious to read. We have undertaken them, in these two instances, for the light that they and the comparisons resulting from them may throw upon the act-systems in general. And we note, first, that they raise, pretty definitely, the doubt whether intentionalism is adequate to the whole subject-matter of psychology. That, to be sure, is a large question, which we could not, in any event, seek to answer at this point: we note only that it is raised, and raised at the very outset, by the act-systems themselves. Stout appears to have transgressed the boundaries which he originally accepted, and Lipps saves the principle only by adding an implicit to his explicit intentionality. In another way, however, the summaries afford us positive light,—light upon the attitude and interest of the authors of these systems. It is clear that the interest lies in argument and discussion and explication and distinction, in the logic of system, rather than in the facts of observation.

It is, indeed, nothing less than illuminating to read Stout's editions with an eye to facts. The third edition has sought to bring its references down to date, so that Sherrington replaces Foster and Myers replaces Ebbinghaus. But the writer's factual equipment has increased hardly at all. 'Views' are what Stout is concerned with, the critical discussion of other men's views and the exposition of his own. The whole vast field of experiment, with its perplexing entanglements of dependence on conditions and theoretical bias and degree of training of observers and all the rest,—this whole bulk of raw material for the science of psychology is passed indifferently by, for what Locke and Hume and Lotze, and Ward and James and Ladd and Marshall and Stout himself 'think' about psychology. Even where, as in the instance of experiments upon the lower animals, Stout refers to monographic sources, even here his attitude is not primarily that of the man of science, careful of method and wary of generalization; he is interested rather in the inferences that have been drawn from observation, in the systematic setting of the facts, in their interpretation and explanation. And as to Lipps! the student of Lipps will hardly realize that there may be, within the compass of psychology, facts of the same existential order that he has met in his study of physics and biology. He reads of unutterables, indescribables, indefinables, uniques, which he is required to 'experience;' and he reads through a serried array of imperatively dogmatic statements regarding these ultimates, which he is required to accept.

Should he wish to go further, he is referred to other works by Lipps himself.[147]

We may grant that Lipps' opinions, and Stout's too, are heartily worth knowing; we may grant also—nay more, we shall insist—that logical construction has its necessary place, is (so to say) the full half of a scientific system. Only we cannot forget that the half here is less than the whole. Moreover, we see from our summaries that the statements originally made, whether dogmatic or argumentative, are sadly instable. Ward once remarked that systematic psychology "is not liable to change every half-dozen years."[148] What is it then, in this psychology of act, that does change? Something changes: simple apprehension is superadded upon cognition, the pure I is ruled out and invited in, perception drops from the judgmental to the prejudgmental level, contents are and are not experiences, sensation has and has not an act of its own, and so forth. Here, surely, are systematic changes! Nor would they be open to criticism if they reflected and kept pace with the growing store of facts, and if the facts were set out at large as ground and warrant of the changes. In the absence of grounding facts, and in view of the general trend and tenor of their work, we must conclude that the psychologising of Stout and Lipps is, essentially, a matter of applied logic. They begin with certain empirical concepts,—the objective reference of consciousness, the conscious I; they proceed to explicate these concepts as thoroughly and minutely as they can; and the longer and more earnestly they meditate, the greater is the wealth of discoverable meaning, the greater the number of its discriminable aspects. This, then, is the positive light that our summaries throw upon the act-systems.[149]

(2) The mention of act and content of sensation brings us to a second point. There can be no possible question that sensation —however it is to be defined; and we need not, for a long time to come, enquire too curiously about its definition—has been, from the beginning, a source of real difficulty to the act-systems. The story is roughly told in the following table:

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[147]The third edition of the *Leitfaden* refers the reader to three books written by other authors: for sensation and fusion to Wundt's *Physiol. Psych.*, for tonal fusion to Stumpf's *Tonpsych.*, and for memory to M. Offner's *Das Gedächtnis*, a work which appeared while the new edition of the *Leitfaden* was in preparation.

[148]*Mind*, N. S. iii., 1894, 143.

[149]It may be added, in the sense of our previous discussions, that the scientific psychologist, whose addiction to fact may leave him neglectful of his logic, has a good deal to learn from this explication of concepts. I have sometimes been staggered to read what my own 'sensationalism' logically 'implied,' when I neither admitted the sensationalism nor acknowledged the implications. All the same, such logical criticism is salutary.

Sensation	Act	Content
Brentano, Höfler, Alexander	Psychical	Physical
Witasek, Geyser	Psychical	Psychical
Stumpf	Psychical	Phenomenological
Lipps, Husserl, Messer	None	Psychical

The table, as a mere outline, obscures many differences. Brentano identifies content with object; Höfler has an analogue to sensation proper in the ideation of a simple psychical, a psychological element; Alexander regards all psychical acts as acts of conation; and so on. The table takes account, too, only of certain systems in which the term 'act' is systematically employed. It omits Münsterberg's 'noetic relation,' and Stout's 'presentative function,' and so on. It shows, nevertheless, how real the difficulty is. The one thing certain is that, somewhere in the world, we come upon sensory contents; and then we must decide for ourselves whether they are physical or psychical or neither physical nor psychical. And we have not even so much of assurance as regards the sensory act.[150]

The doubt raised by our summaries, whether intentionalism is adequate to the subject-matter of psychology, seems therefore to be well founded. For if one starts out with intentionalism one can hardly find anything simpler than the perception of external objects. But then one is reminded, whether historically or empirically, that there is something logically prior[151] to perception, namely, sensation; and yet sensation is not obviously intentional. What, then, is to be done? Well, one may speak of intentional consciousness as 'consciousness in the pregnant sense,'—whatever that may mean; one may draw a distinction between matter and form of consciousness,—as if form were in some way a guarantee of intention; one may oppose 'potential' to 'actual' intention,—whatever, again, that may mean. Or one may throw the sensory content overboard, and keep the sensory act as a mode of perception or ideation or conation. Or one may hold fast to the letter of intentionalism, and make the sensation, act and content together, an humbler understudy of perception. It is a matter of taste which course one adopts, and it is a matter of skill how well the resulting system holds together. Whence of course it follows that no degree of subjective assurance and no refinement of critical acumen

[150] A. Höfler, *Psychologie*, 1907, 210; S. Alexander, "On Sensations and Images," *Proc. of the Aristot. Soc.*, 1910, N. S. x., 1910, 1 ff.; J. Geyser, *Lehrbuch d. allg. Psych.*, 1912, 49, 224, 306; H. Münsterberg, *Grundzüge d. Psych.*, i., 1900, 309; Stout, *Manual*, 1913, 210.

[151] Or, perhaps, chronologically prior; sensation, in certain systems, still has a genetic flavour. Cf. H. Hofmann, "Untersuchungen über den Empfindungsbegriff," *Arch. f. d. ges. Psych.*, xxvi., 1913, 1 ff.

on one's own part can prevent a like assurance and a countering criticism on the part of others. But a house divided against itself shall not stand.

(3) After all, though, it may be said, we have not proved that the 'house' is divided. Sensations lie on the outskirts of psychology, form the ragged edge of the psychological system; we meet them at the outset, but we have very little to do with them thereafter. Besides, the difficulty, such as it is, may readily be cut; it has been cut cleanly enough by Stumpf, who dismisses all the doubtful elements, all sensory and imaginal contents, to a limbo of their own. Why should we lay so much emphasis upon a merely preliminary difficulty?

The objection forgets that we are talking of system, and that a system must be systematic throughout. It forgets that the diversity of opinion among the psychologists of act is due precisely to their effort toward a consistent systematization. Their chief interest is here, on the side of applied logic; and a break-down at the beginning is, logically, as serious as a break-down later on. We need not rest, however, with this reply. We will go to the heart of the systems, to the doctrine of attention; and we shall find that attention, no less than sensation, is a stumbling-block to the intentionalist school.

We hasten to make an exception of Lipps; but then Lipps' whole system is exceptional. It embodies, so to say, two psychologies, real and phenomenal, unconscious and conscious. Every real psychical process has, according to Lipps, an intrinsic energy, in virtue of which it attracts or appropriates psychical force. Attention, now, is a term which belongs in strictness, not to consciousness, but to the domain of the real mind: it is nothing else than the psychical force which, accruing to a real process, lifts it (under favorable conditions) above the limen of consciousness. We then 'have' a conscious content. If the process appropriates still more force, or if attention turns to it in greater degree, it becomes a process of thought, and we have in consciousness the activity of apprehension which culminates in the simple act of thought. Here is the intellectual limen. If the process is capable of yet further appropriation, it becomes an apperceptive process, and we experience in consciousness the activity of apperception, which results, according to circumstances, in various acts of higher intellectual orders. Attention, throughout, is the psychical force which 'turns to' or 'is appropriated by' the real process of ideation, of thought, of apperception.

No one will deny that this doctrine of attention is logically constructed. Our objection, if we object at all, can only be that it rests upon a basis of pure invention. That is for Lipps neither an objection nor a difficulty; he insists that invention is neces-

sary, and that his own "substructure of thought" is adequate to the psychological occasion. If, as he admits, we know nothing of process and stage of process, of psychical energy and psychical force, that truly is our misfortune; but we may then be all the more grateful to thought for supplying the deficiencies of knowledge.[152]

Such is the exceptional system, for which attention has no terrors. The rest are less happy. Stumpf, as we know, identifies his primitive function of perceiving with a 'taking note of;' attention thus seems to be present to consciousness from the first. This perceiving has a graded attribute of distinctness (*Deutlichkeit*), which Stumpf nevertheless trusts so little that he is forced to speak figuratively of an 'accumulation of consciousness.' Messer, getting no help from Husserl, turns to Lipps; attention is an attitude of the I, logically prior to our consciousness of objects,—and all consciousness is consciousness of objects. Witasek, in flat contradiction to the school of Husserl, makes attention an act, one of the ubiquitous acts of judgment. "To many contemporary psychologists," he adds, "this opinion will appear nothing less than monstrous; all the same it is true; and anyone who has a discerning eye for the psychological specificity of the act of judgment will recognize it without difficulty in the constitution of attention." Geyser, who paraphrases attention in Lippsian terms as our "intellectual occupation" with an object, must transcend consciousness in both directions in order to bring his subject under control. Attention as psycho-physiological energy is responsible for the clearness of certain conscious contents; and attention as the intellectual occupation of the mind with the contents of consciousness (this 'mind' is a matter of supplementary inference, not of observation) is responsible for our reflective fixation of them. So there are two attentions, and neither is psychological. Pfänder, like the Messer of *Empfindung und Denken*, makes attention the higher degree of our consciousness of objects, the denser or more concentrated portion of the cone of light which issues from the I of consciousness and plays upon its immediate objects. Finally, Stout retains in all three editions the statements that "attention is simply identical with conation considered in its cognitive aspect" and that "conation and cognition are different aspects of one and the same process," statements which, in default of some equivocation, would seem to be irreconcilable.[153]

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[152] *Leitfaden*, 1909, 78-83, 141-148.

[153] Witasek, *Grundlinien*, 297; Geyser, *Lehrbuch*, 256 ff., 261 ff., 724 f. See esp. 263: "This reflective fixation does not represent the consciousness-of, *i. e.*, is not a mode of awareness, but is a holding fast of the content of which we are conscious, to the end that the mind energise on this content its acts of relating, and thereby extend contentwise its awareness of the content."—Stout, *Manual*, 1899, 247, 581; 1907, 257, 599; 1913, 367, 704; A. Pfänder, *Einführung in d. Psychol.*, 1904, 272 ff., 354 ff.

It was plainly a bad day for empiricism when the experimental movement brought attention to the forefront of systematic psychology.[154] Intentionalism can deal with perception and imagination and memory and thought and emotion and desire, but hardly with attention. As in sensation it finds too little, so in attention it finds too much. For what is attention, empirically taken, if it is not already and of its own nature intentional? It too is a surplusage, and so it suffers a fate akin to the fate of sensation. Either it is thrown out of consciousness, not (to be sure) into physics or phenomenology, but into a realm of logical priority; or else it is identified with some particular intentional process. And the extremes meet. Perception, which at its simplest is for Höfler and Witasek sensation, becomes for Stumpf, still in its simplest form, an implicit attention.

§14. It would seem then, that the differences among the act-systems are in fact fundamental and inevitable, not superficial and accidental. On the side of subject-matter, intentionalism cannot cope with sensation and attention, while it cannot either dispense with them. Witasek, it is true, takes heroic measures; sensation is perception, and attention is judgment: the system is saved. But who, outside of Meinong's school, will accept a salvation offered on such Procrustean terms? Besides, the interest in systemization, in applied logic for the sake of the logic, characterizes all the psychologists of act. Psychology appeals, so to say, to their personal ingenuity in relating and distinguishing and constructing; and where the appeal is thus individual, there—as in philosophy or poetry—the outcome will of necessity reflect the personality of the writer. We saw that there are many differences between Witasek and Messer. We may now safely say that these differences go deep. They are the differences, not of two scientific psychologists, but of two personalities expressing themselves in the terms of systematic psychology. If intentionalism is scientific, then science can no longer be called impersonal.

How indeed shall we account, otherwise than by personality, by training induced upon given temperament, for the varying definitions of the 'act' itself? An act for Lipps is a doing, the deed of the I of consciousness. The picture that rises from his pages is that of a strenuous and resourceful, highly self-conscious 'individual,' acting and reacting in a world of other individuals and of material things. An act for Husserl is something very different, something that by contrast almost suggests passivity: an experience of a certain essential constitution, of intrinsically

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[154] Cf. my *Feeling and Attention*, 1908, 171 ff.

intentional make-up. Husserl accordingly reminds us of nothing so much as the skilled lexicographer, teasing from the word before him every discriminable shade of meaning, and nicely distinguishing it from the words that everyday use has made us think synonymous. Stumpf's act, lastly, lies between these other two. It is active, in the sense that it is found or given as active; it is by no means the deed of an I. It is an active verb, moving amidst phenomena and relations, and generating its 'correlate,'—a sort of caddis-worm that houses itself variously in the sticks and shells and stones of its independently variable surroundings. We need not go further. We have an act which is my doing and is experienced as my doing; we have an act found as active ultimate among inactive ultimates; we have an act which is an embodied intention, the subject-matter of a morphology of knowledge. What, then, for psychology, is 'the act?' We are brought back, after all, to our polemical starting-point: there is no psychology of act, there are only psychologies. But we may now add, as at the beginning we could not, that on the basis of intentionalism there will be only psychologies.

Here, however, we remark a notable difference between the psychology of act and the psychology of function. There is no reason to suppose that functional psychology enjoys any long lease of life. It was born of the enthusiasm of the post-Darwinian days, when evolution seemed to answer all the riddles of the universe; it has been nourished on analogies drawn from a loose and popular biology; it will pass as other fashions pass. Even now, indeed, it may be passing. The movement that has labelled itself 'behaviorism'—a 'psychology' not only without a psyche and a psychical, but also without a psychological—appears to get its motivation, at any rate on the negative side, from dissatisfaction with the psychology of function.[156] But be that as it may, there is no seed of life in functionalism compared with the power of perennial self-renewal that inheres in intentionalism. Functional psychology (if we may again change the figure) is a parasite, and the parasite of an organism doomed to extinction, whereas intentionalism is as durable as common sense. We noted long ago that the empirical psychologist (we may now

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[156]Behaviorism has not yet become clear either as to its own working concepts or as to its relations to psychology: see A. Robinson, "Behavior as a Psychological Concept," *Proc. Arist. Soc.*, N. S., xviii., 1918, 271 ff. A reaction against functionalism is suggested by the biological flavor of behavioristic writings, and is expressly admitted by J. B. Watson (*Behavior, an Introduction to Comparative Psychology*, 1914, 8 f.). Logically, indeed, a strict behaviorism can have no quarrel with an existential psychology, since there is no point of contact between the two disciplines. The only possible relation is that of correlation, and the extreme behaviorist declines to correlate. Cf. my critique of Watson, "On 'Psychology as the Behaviorist Views It,'" *Proc. Amer. Philos. Soc.*, liii., 1914, no. 213.

say, the psychologist of intention' means to take mind as he finds it, and that like all the rest of the world, who are not psychologists, he finds it in use: he finds it actively at work in man's intercourse with nature and with his fellow-man, and in his discourse with himself.[156] That is how 'mind' naturally presents itself to common sense, to the man of affairs, to the intelligent man of science who lacks psychological training. A great mathematician and physicist, speaking in 1869 of the "phenomena of mind," declared that "science can be expected to do but little to help us here, since the instrument of research is itself the object of investigation:" since (that is to say) the mind which we study is the mind by which we study, or the intentional experiences which we seek to know are the intentional experiences whereby we know. And if it is objected that fifty years have allowed a good deal of water to flow under our scientific bridges, we may point out that Stokes' words are repeated by the physicist Tait in 1885 and by the biologist Thomson in 1911.[157] They would be accepted today, without objection or reflection, by the vast majority of scientific men outside of psychology itself. Small wonder, then, that within psychology too this same common-sense attitude, an attitude natural to us as our mother-tongue, should never fail of representatives! We shall always have psychologists of Brentano's stripe: what we have tried to make clear is that these men will give us psychologies, but not (as Brentano hoped) psychology.

These conclusions may content us. In showing that intentionalism takes the obvious, natural, proximate, common-sense view of psychology and psychological problems, and that the adoption of this pre-scientific view as scientific puts a premium upon individual differences, upon personal ingenuity of explication and arrangement, we have probably done as much as by mere counter-argument we are able to do. It would be useless to write out, over against the psychologists of act, the list of those who deny that they find intentional experiences in the contents of consciousness, for the affirmative is always in better logical case than the negative. Moreover, the denial itself shifts the universe of discourse, or changes the point of view from which 'consciousness' is regarded. When Ach tells us that an observer reports, from the fore-period of a simple sensory reaction, one knowing (*Wissen*) and three to five awarenesses (*Bewusstheiten*), we may perhaps be surprised by the fullness of the report, and may even go so far as to suspect the influence of suggestive

[156] This JOURNAL, xxxii., 1921, 119.

[157] G. G. Stokes, Presidential Address, in *Report of the 39th Meeting of the Brit. Assn. for the Advancement of Science*, 1870, cv.; P. G. Tait, *Lectures on some Recent Advances in Physical Science*, 1885, 26; J. A. Thomson, *Introduction to Science*, 1911, 105.

questioning; but we do not meet the situation by declaring that "a knowing (*Wissen*) is never given in consciousness;" we have then simply substituted our own definition of psychology for that of Ach.[158] The one complete and positive reply to intentionalism is the existential system, the system that is partially and confusedly set forth (anything like completeness and purity of exposition is not possible to our present knowledge) in the works of Wundt and Külpe and Ebbinghaus.[159] If we can build psychology upon a definition that is scientific as the word 'science' is to be understood in the light of the whole history of human thought; and if we can follow methods and achieve results that are not unique and apart but, on the contrary, of the same order as the methods and results of physics and biology; then, by sheer shock of difference, the act-systems will appear as exercises in applied logic, stamped with the personality of their authors. They will not, on that account, languish and die, because 'mind in use' will always have its fascination, but they will no longer venture to offer themselves as science.[160]

§15. Negative criticism always needs more words than positive construction. The upshot of the preceding paragraphs may, however, be condensed into a brief statement. The claim has been made that 'conscious' phenomena constitute a special

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[158] N. Ach, *Ueber die Willenstätigkeit und das Denken*, 1905, 40 f.; K. Marbe, *Experimentell-psychologische Untersuchungen über das Urteil, eine Einleitung in die Logik*, 1901, 92; cf. G. E. Müller, *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, iii., 1913, 542; A. Messer, *Experimentell-psychologische Untersuchungen über das Denken*, *Arch. f. d. ges. Psych.*, viii., 1906, 207 *n*. It is needless to say, after the criticism made of Ladd's psychology, that I agree with Marbe; I am here concerned with formal argument.

[159] I am thinking, of course, of the earlier Külpe ("Das Ich und die Aussenwelt," i., *Philos. Studien*, vii., 1892, 405; *Grundiss der Psychol.*, 1893, 27), and of Ebbinghaus before his work was edited by Dürr.

[160] In *Thought-processes*, 60 f., I sought to 'psychologise' Brentano's act as being, existentially, the temporal factor intrinsic to psychological subject-matter. Thereupon a friendly critic remarked: "On peut s'étonner que Titchener...ait cru devoir exposer et discuter des théories dont la valeur psychologique est douteuse. Il s'agit plutôt d'analyses verbales, d'idéologie, de subtilités, de distinctions scolastiques" (T. Ribot, *Rev. phil.*, lxix., 1910, 650). My attempt sprang, indeed, from a rather desperate desire somehow to bring intentionalism and existentialism together at close quarters, and to transcend the mere calling of names. If the two schools were in any real sense schools of psychology, then—I thought—they must after all be concerned at bottom with the same problems; and I knew that the epithets 'scholastic' and 'sensationalist' were often applied ignorantly and unintelligently. The same year, 1910, gave us, however, Wundt's essay on "Psychologismus und Logizismus" (*Kleine Schriften*, i., 511 ff.), which amply justifies Ribot's reproaches.

class of objects of experience, immediately and radically distinct from phenomena that are not-conscious, and that the science of psychology has to do with the objects of this given class. The resulting systems are either functional or intentional. We have found that in both cases they are empirical, that is, technological: they begin and end with 'mind in use.' They represent what we may call an art of mental living as distinguished from a science of mental life,—a general 'applied psychology' that is logically prior to the special 'applied psychologies' of education, vocation, law, medicine, industry. Functional psychology is through and through teleological, and by biological analogy lays down general norms, either directly or through the intermediation of philosophical theory, for the right conduct of our practical life. Intentional psychology is at once more individual and less naive than functional. We may perhaps say that its central task is logically to analyse, to explicate, the operations of perception and thought, as these terms are understood by the average educated person or are received from philosophical tradition; that it extends this procedure of logical analysis to emotion and will, understood in the same way; and that it seeks finally, with marked individual difference, to base the whole of psychology upon the intentional principle. It is thus, like common sense, an applied logic, though unlike common sense its interest lies more in the logic and less in the results of application. Hence it has a natural affiliation to philosophy, and especially to theory of knowledge or pure logic. Since, however, it is not itself pure logic, but rather a logical account of 'psychical phenomena,' it stands also in close relation to the particular technologies of mind, and especially to education.[161]

We see, then, that these 'psychologies of consciousness,' in order to maintain a logical continuity with philosophy above and everyday practice below, sever psychology from the other sciences, and redefine 'science' to suit their case. We can understand how philosophy, while wholly unconscious of bias, should look with favor upon such systems and with disfavor or indifference upon a truly scientific psychology. We can understand, too, how it comes about that current philosophy should have much to say concerning psychology, and but little to say of physics and chemistry. We can understand that psychiatrists and educators, eager to turn psychology to practical ends, should appeal to systems that are already technological and should look impatiently away from the bare impersonal facts

[161] I am here characterizing technology *a potiori*, by reference to the pure science upon which it preponderantly draws. In strictness such characterization is not permissible.

of an existential science.[163] All this we can understand, and understanding takes off the bitter edge of controversy. But we see, on the other hand, that physics and chemistry, and of late years biology also, are going their theoretical way without looking aside either to philosophy or to application. We see that they are achieving results of which philosophy must, in the long run, take account; and we see that these results are at once finding technical application. All this, therefore, is ground of encouragement to the votary of a strictly scientific psychology. And if our negative criticism is valid, then the feeling of encouragement becomes an imperative 'experience of claim.' Psychology fairly challenges us to attempt its systematic exposition on an existential basis.

[163] The appeal is intelligible in the light of history and of the historically conditioned education that these technologists receive. Yet it is worth remembering that there is no general technology of physics or chemistry or biology, to mediate between the sciences and their special technologies, the special branches of engineering and medicine. Remote and aloof from everyday life as the laboratories are, their results are taken up into practice at first hand. It is worth remembering too that, despite all the psychological systems from Aristotle down, it is only since the appearance of experimental psychology and its attainment of impersonal results that the special technologies of mind have sprung into vigorous being. Cf. G. E. Müller, *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, i., 1911, 147; H. Münsterberg, *Psychology and Social Sanity*, 1914, 291 ff.

AFFIRMATION AND NEGATION

By C. H. Griffitts, University of Michigan

My purpose here is to report the results of four experiments dealing with the processes of affirmation and negation, and not to review or criticize the literature on the subject.[1] However, as a sort of a text or point of departure, I have chosen the following from Breese's *Psychology*, p. 323.

"In this conection we should note that the so-called negative judgments, or judgments of disbelief, are negative only in the form of language expression. The judging attitude itself is always a positive one. For instance, on seeing a counterfeit bill, I may exclaim, 'It is not good'. The judgment here consists in my acceptance of the represented content as 'not good'. This acceptance is psychologically a positive attitude, although the expressed form is negative. The judgment involved in the sentence 'The table is not round' is my positive acceptance of 'not-roundness' as an attribute of the table. Psychologically there are no negative judgments any more than there are negative perceptions, or images, or memories. It is only from the logical point of view that judgments may be called negative, and then only because of their external form."

The four experiments to be described in this report were devised to test this theory regarding negation, a theory which one finds expressed frequently in logical and psychological literature. Stated briefly, these experiments were:

I. Reaction-times: colors. Colored cards were shown in an exposure apparatus, the subject being instructed to move a lever in one direction if the color were red, for example, and in the opposite direction if the color were not red.

II. Cancellation of letter groups. Printed sheets containing 100 groups of four letters each were handed to the subjects. On some they were to cancel those groups containing specified letters, on other sheets those groups in which none of the specified letters appeared.

III. Reaction times: multiplication equations. Multiplication equations were typewritten on cards to fit the Ach exposure apparatus. The subject was instructed to move the reaction-lever in one direction when the answer was correct and in the opposite when it was incorrect.

IV. Cancellation of paired letters. 225 pairs of letters were mimeographed on each of the five sheets given to each subject. On some they were to cancel those pairs in which the two letters were the same, on others those pairs in which the two letters were different.

Experiment I.

A reaction-time set-up of apparatus was used in this experiment. It included an Ach card-exposure apparatus for 4 x 8 cm. cards, a Hipp or pendulum chronoscope, and a special reaction

[1] A brief bibliog raphy is given at the close of this article.

instrument so made that movement of a lever either to the right or to the left would either make or break the circuit, as desired. Several different sets of cards were used, each of which contained fifty cards of some one color, and ten each of five other colors.

The one color which appeared fifty times in the set was shown to the subject, who was instructed to react in one way whenever that color appeared and in the opposite way whenever any other color appeared. These other colors were not named. The subject was told that half of the cards were of the color shown him. These cards were shuffled together in the presence of the subject, but behind the exposure apparatus. With several of the subjects several different sets were used, on different days. In half of these experiments the positive[3] reactions were to the left and the negative to the right; in the other half this relation was reversed.

Results. The average reaction times for the positive and negative reactions and the differences between them are shown in Table I. Results are given for each day's work with each subject. While there was naturally some variability in the reaction time for each subject, the results are so uniformly in favor of the positive reactions that it was not thought necessary to compute any measure of variability.

These results furnish fairly conclusive evidence that the positive reactions take place more quickly than the negative. When red, for example, is the positive color, the processes including the appreciation of red and the appropriate reaction take place more quickly than those which include the appreciation of some other color as not red and the appropriate reaction.

TABLE I

AVERAGE REACTION TIMES, IN SIGMA

Subject	Positive	Negative	Difference (N–P)
Spr	328	400	72
Spi	317	424	107
..	284	386	102
..	266	325	59
..	245	370	125
..	276	363	87
Pee	369	469	100
..	364	458	94
..	360	451	91
Thi	321	403	82
..	355	374	19
Kuh	280	281	1
And	389	412	23
Gri	320	336	16

[3]Reactions to the one specified color will for convenience be referred to as "positive" reactions, and reactions to the other colors as "negative" reactions. The colors may also at times be called the "positive" and "negative" colors.

The introspections show great individual differences with regard to the method of fixing the instructions in mind and also with regard to the attitude toward the problem itself. For example, one subject made two reactions in succession when negative colors were shown, and this caused her to focus her attention for some time on the negative reaction. Her adjustment during the fore-period was so favorable to the negative reaction that results for the rest of the experiment showed shorter reaction time for the negative reactions.

At first there is a tendency to keep the positive color in mind, verbally or otherwise. The retention of the instructions as a whole may be wholly in verbal terms, in a combination of verbal imagery and kinaesthesis (arm and hand), or in a combination of visual imagery of the positive color and kinaesthesis. As the experiment proceeds there is a tendency for the instructions gradually to fade from consciousness, the reactions becoming more and more automatic. However, the adjustment of the nervous system probably remains much the same, so long as the subject does not notice just how many and just what the negative colors are.

There seems to be a positive relation between the tendency to visualize the positive color and the difference between the positive and negative reactions. Subject Spi has exceptionally clear and projected visual imagery. He reported that during the fore-period there was nearly always an image of the positive color projected to the slide covering the cards.

No concrete visual image of "not-red" was ever reported.

Psychologically, it seems to make little, if any, difference whether the consciousness that blue is not red, and therefore to be reacted to in a certain way, be verbalized as "this is not-red" or "this is-not red." Psychologically, either is a negation, at least so long as there are several not-red colors. The starting point in either the positive or the negative reaction is with "redness." "Red," particularly for the concrete-minded individual like Spi, is the "idea in mind." If the color shown is the same as this idea in mind, the judgment or perception—let us not quarrel over terminology for the present—may be regarded as positive, affirmative, or one of similarity. If the color exposed is not red, or not-red, the starting point is still with regard to red or redness, with a consciousness that the color when it appears is not in the class of reds.

Neurologically, this means that there is a definite adjustment beginning with the processes involved in the appreciation of red, and leading over to the appropriate reaction, so that when red appears the action takes place automatically, or almost so. But since each of the colors is specific, and since "not-redness" cannot be represented concretely, there can be no

complete sensori-motor adjustment for the negative colors. The adjustment on the motor side may be as complete, but not on the sensory side.

I suppose that no one would deny that there is some sort of retinal-cortical reaction of a certain more or less specific nature involved in the perception of red as red, and that the sensori-motor adjustment involved in reacting to red includes this retinal-cortical reaction or process. I believe it probable that the reactions to the negative colors under the conditions of this experiment must also involve this same process. If, during an experiment, something were suddenly to happen to this red-perceiving process, it is likely that it would prevent correct reactions to either red or not-red stimuli.

In opposition to the above interpretation or explanation of the results of this experiment it is possible that they are due, in part at least, to the fact that there were five times as many reactions to the one "positive" color, as to any one of the "negative" colors, and that during the course of the experiment there was a gradual development of specific reaction-habits to each color. If so, it is obvious that the mechanization of the sensori-motor processes starting with and including the appreciation of the positive color would take place more rapidly.

To modify this experiment in a way that would enable us to determine whether the results reported above are the result of, or affected by, the formation of any such specific reaction-habits, it would be necessary to meet two conditions. It would be necessary to have, first, as many of each of the "negative" colors as of the one "positive" color; and secondly, the total number of positive and negative reactions would have to be the same. It is obvious that this would be impossible. Other practical difficulties arise when one starts to work with a number of different colors all to be regarded as positive and an equal number to be regarded as negative. These two conditions, however, were met in the next experiment.

Experiment II.

My purpose in this experiment was to determine whether the correct explanation of the results of Experiment I was made, or whether those results were due to the establishment of reaction-habits to each of the colors used. To do this, it is necessary (1) that the number of positive and negative reactions be the same, and (2) that the number of elements to be regarded as positive be equal to the number of those to be regarded as negative.

Two carefully mimeographed forms were prepared, each having 5 columns of 20 groups of letters, each group containing 4 letters. Only 8 letters appeared on any one form, and different letters were used for the two forms. Each column in the first form contained 4 groups composed of the letters B R D X, in different orders, and 4 groups composed of the letters N J S T; 4 groups containing three of the first group of letters and one of the second; 4 groups containing two letters of each; 4 groups containing one of the first and three of the second. Taking the whole page into consideration, each letter appeared an equal

number of times. Each letter also appeared an equal number of times in each of the 4 positions in the groups. There were some slight deviations from the last two of the above rules, on account of the fact that there were only 8 letters and 100 groups. The groups in each column appeared in chance order. There was double spacing between the letters in the group, and between successive groups in the columns. To illustrate the nature and arrangement of this material, the groups in the first column were as follows:

D N X J	D S T R	B D R X	B S J N	T N J D
B D X T	D R X B	N R T S	T S J N	S J T N
S J D T	T N J S	R B X D	R X B D	D R J X
B J R X	J T S N	S D X B	S X R N	S R B J

No two groups were identical; at least the order was different.

The second form contained the letters H Z C K and L F M P. The general arrangement was the same as in the other form.

The experiment was divided into four parts, in each of which both forms were used. In Part A, the instructions for the use of the first form were: "When I say 'start', turn over the first sheet, and working by columns, cross out every group of letters which contains all four of these letters B R D X. (These letters were then written on the blackboard.) Work as rapidly and accurately as you can. After 45 seconds I shall rap on the table, and you will then turn immediately to the second sheet, and on this sheet cross out all those letters which contain none of the letters named." Then the next form was used, the instructions being the same with the exception that on the first sheet the subjects cancelled the groups which did not contain any of the letters H Z C K, and the second sheet those which contained all those letters.

With the second group of subjects this whole procedure was reversed.

Part B. The same forms were used, but the subjects were given only three letters to keep in mind, these being R B D for the first sheet and C H K for the second.

Part C. In this part only two letters were given, these being B X for the first, and K Z for the second form.

Part D. Here only one letter was given. This was the letter D for the first form, and the letter H for the second.

With the first group of subjects these different Parts were given in order just the reverse of that in which they are described above, Part D being given first and A last. With the second group this order was followed for the first half of each Part, and reversed for the second half. Yet, for any one of the Parts, the order, neglecting interpolations for the first group of

subjects, was I–P, I–N, II–N, II–P, I and II referring to the form used, and P and N to the nature of the instructions. With the second group the order was I–N, I–P, II–P, II–N.

For convenience, in what is to follow, the groups of letters containing all the letters given in the instructions will be called "positive" groups, and those which contain none of these letters will be called "negative" groups. The cancellations of these groups will also be referred to as "positive" and "negative."

In each of the four Parts, the number of possible positive and negative reactions was the same.

Not only was the number of positive and negative reactions the same, but, in Part A, as the vision was directed successively to one after another of the groups, the four letters given in the instructions and to be regarded as positive would appear the same number of times as the other four letters; and altogether, each letter would appear the same number of times as any other letter. The results of Part A are therefore more conclusive than those of the other three, so far as the interpretation of the results of Experiment I may be concerned.

With the first group of subjects each work period was of 30 sec. duration, while for the second group it was 45 sec. To make the results more directly comparable with each other and with the results of the other experiments the scores for each group were recorded in terms of the number of groups inspected, omissions, and errors per minute.

Results. The averages of the results for the two groups of 42 subjects are shown in Table II.

Little comment is needed. It is evident that the presence of a letter or of a group of letters, even with the order changed, is more quickly detected than the absence of these same letters from a group. These results seem to justify the explanation offered for the results of Experiment I; and, if so, that explanation would apply here as well as there. In fact, I cannot see how the results of this experiment can be explained in any other way.

TABLE II

CANCELLATION OF LETTER GROUPS: AVERAGE SCORES PER MINUTE

Part of exper.	Positive Instruction			Negative Instruction		
	Speed	Omissions	Errors	Speed	Omissions	Errors
A................	88	1.06	.10	53	.45	.70
B................	62	.41	.03	50	.36	.46
C................	77	1.33	.22	62	.39	.45
D................	104	2.65	.37	72	.21	.31
Average..........	83	1.36	.18	59	.35	.48

The subjects were asked to write out their introspections, ... proved to be a pretty hard task, as they had had little ... training in introspection, and nothing was said beforehand ... the purpose of the experiment and they were not ... that introspections were to be called for. Since I was ... concerned with the objective results, I did not care to ... the risk that the setting of an introspective task might interfere with the work itself, which I wanted to be as free and ... as possible under the conditions of the experiment.

Almost without exception, the subjects reported that it seemed easier to detect the presence than the absence of a letter or group of letters. As to the reason for this, many said that the letters they were looking for seemed to stand out more clearly than the others, that they seemed to catch the attention more quickly. In Part D, where just one letter was given in the instructions, some of these subjects reported that the letter they were looking for seemed to stand out, while the other letters in the group were not noticed." The explanation for this is of course to be found in the nervous basis of attention; that is, in an adjustment of the nervous system to facilitate the reception of the stimulus corresponding with the "idea in mind."

An interesting fact appears in the relative number of omissions and of wrong cancellations under the two working conditions. There are more omissions and fewer wrong reactions ... positive instructions. Not only that, but under positive instructions there are more than seven times as many omissions ..., while under negative instruction there are generally ... errors than omissions.

Experiment III.

The apparatus used was the same as that of Experiment I. A ... multiplication equation, 9 x 4 = 36, for example, was typewritten on each of the cards, 4 x 8 cm., cut to fit the Ach card-exposure apparatus. On half these cards the answer given was wrong. Great care was taken to insure equal average difficulty of the equations with and without correct answers. In half the experiments the subjects moved the stick to the right when the answer was correct, and to the left when the answer was incorrect. In the other half the procedure was reversed. The subjects were told that half the answers were correct. The cards were shuffled together in the presence of the subject. There were 125 cards in the set, and none of the two left-hand members of the equations was larger than 9.

When the answer was wrong, it was always wrong by just ..., and for one-half it was larger, and for one-half smaller, than the correct answer.

In his introspective study of the process of negation, Wolters[3] used arithmetical equations in which there was a great difference between the given and the correct answers. He did not measure reaction times.

Results. The results are presented in Table III. They furnish rather conclusive evidence that it is easier to perceive the correctness than the incorrectness of the right-hand member of a multiplication equation. The situation confronting the subject and determining his attitude and adjustment in the fore-period is different from that in the preceding experiments, for in them each successive color or group of letters was judged with reference to a certain specified color, letter, or group of letters. In this experiment some of the subjects at first repeated (inner-speech) the substance of the instructions just before the card was exposed. This inner-speech repetition of the instructions tended to disappear as the experiment progressed.

During the mid-period (between the exposure of the card and the reaction) some subjects reported a tendency to read the equation with an inner-speech accompaniment. Many times the inner-speech recitation of the answer was not completed, although it might be begun. If the answer on the card was the number learned and associated to the two left-hand members of the equation, the whole process including the appropriate reaction seemed to take place smoothly and with little effort.

Some subjects report no inner-speech at all during the mid-period. One subject, Spi, reported some visual images of the correct answer to the equation in those cases where the answer on the card was wrong.

TABLE III

AVERAGE REACTION TIMES IN SIGMA

Subject	Positive (correct answers)	Negative (incorrect answers)	Difference (N-P)
Spi	1074	1341	267
..	1039	1439	400
Thi	1126	1270	44
Kuh	1010	1060	50
..	911	988	77
Gri	685	865	180
Students' results[4] Av. 21 subjects	913	1058	145

[3]Wolters, A. W., The Process of Negation, *Brit. Jour. of Psychol.*, 8, 1915, 183-211.

[4]These results were obtained by students working under my immediate direction and observation. Since I carefully refrained from any expression of opinion as to what the results might show, the results are probably fairly reliable. The differences ranged from 31 to 377 sigma in favor of the positive reactions.

When the answer on the card was wrong, there seemed to be a momentary confusion or disturbance. At least the whole process up to and including the initiation of the reaction seemed not to take place so smoothly and readily as when the answer is correct.

As with the preceding experiments, for our present purpose it makes little difference what we call the mental process involved in determining whether or not the answer on the card is correct. Looked at in slightly different ways, it might be regarded as one of affirmation and negation, as positive and negative judgment, or as the judgment or perception of similarity and difference (or of identity and difference), since it is largely a matter of comparing the given with the correct answer. Considered in this way it really makes little difference whether we say "36 is-not 35" or "36 is not-35." From either the psychological or neurological point of view either is a formal verbal statement of negation, or of a judgment or perception of dissimilarity or non-identity. However stated, the process is more complex than that which might be formally stated as "36 is 36."

If we assume that there is a set of cortical elements or paths, or some cortical process, which is functionally related to our consciousness of "36" (and its meaning), I believe it would be probable that the sudden elimination of this process would prevent a correct reaction whether the card showed "9 x 4 = 36" or "9 x 4 = 35." The mental processes preceding the reaction to the last card would be more complex, on this assumption, since they would include not only the "36" process but the "35" as well. This greater complexity would then explain the longer time required.

However, apart from any particular explanation which may or may not be valid, the bare results themselves furnish evidence that affirmation and negation are not the same; that they are not merely "two sides of the same process," as some one has said. From the standpoint of the action of the nervous system, it is not true that all negation is affirmation.

Experiment IV.

This experiment is one dealing more with similarity and difference than with affirmation and negation. But since these two problems are closely connected I decided to include this with the other experiments.

Some experimental work on this problem has been done. All [illegible] along the same general lines as that done by [illegible] who had his subjects arrange specimens of hand-

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[illegible] Judgments of Similarity and Difference,
[illegible]

writing in order of similarity and of dissimilarity in respect to a standard. Among other results, he found greater variation in the different arrangements in order of dissimilarity than in the other arrangements. It is not my purpose here to review or criticize the experimental work on this problem. Some of the results obtained by other investigators agree and some disagree with those obtained by Hollingworth. A list of references may be found at the close of this article.

Method. The subjects in this experiment were given carefully mimeographed sheets each of which had nine columns of twenty-five pairs of letters each. The letters used were: A, B, D, E, H, I, K, L, M, P. In half the pairs in which A appeared at all it was in first position, and in half in the second. There were ten pairs, both A's; ten pairs with A in first place and some other in the second place; and ten pairs with A in the second position and some other letter in the first. The same general rules apply to the appearance of each of the other letters.

The pairs appearing in the first column will illustrate the material used. These were:

I B	P D	B B	C C	M M
D D	P P	H I	K K	P P
E E	M I	I I	L A	C C
M L	E L	L K	K K	M H
P A	P L	A A	A D	K D

Each subject was given five of these sheets, stapled together. The instructions were: "Turn over the bunch of papers, and on the first sheet mark 'S' at the top of the first column, 'D' at the top of the second, 'S' at the top of the third, and so on, writing 'S' and 'D' alternately at the top of the nine columns. Now turn the sheets face downward again. When I say 'Start', turn the bunch over and, beginning at the top of the first column, cancel all the pairs of letters in which the two letters are the same. When I strike the table, stop working in that column and begin at once at the top of the second column, cancelling the pairs of different letters. Then at the next signal start at the top of the third column, cancelling those that are the same, and so on until the last column has been finished. Are there any questions? Ready. Start." After this page had been finished they were asked to turn the sheets face downward again. The instructions for the rest of the experiment were: "When I say 'Start' turn to the second sheet, and cancel all the pairs in which the two letters are the same. When I strike the table, turn to the second sheet and cancel the pairs of letters not the same; at the next signal turn to the third and do the same, that is, cancel those not the same; at the next signal turn to the next sheet and cancel the pairs that are the same. To help you re-

member which to cancel, when I give the signal each time I will say 'Same' or 'Different'. You will be allowed 60 sec. for each sheet. Ready. Start." Two sec. were allowed each time for turning over the sheets. Two groups of subjects, 42 altogether, were used, the procedure being the same with the exception that the order, exclusive of the introductory or practice sheet, with the first group was S–D–D–S while with the second it was D–S–S–D.

Results. The results of the introductory sheets were not scored for the reason that too many subjects finished each column within the 10 sec. interval allowed.

Table IV shows the scores for speed, omissions,and wrong cancellations or errors. The speed scores represent, not the number of cancellations, but the average number of pairs in-

TABLE IV

AVERAGES OF TWO ONE-MINUTE WORK-PERIODS

	Letters the same			Letters different		
Group	Speed	Omissions	Errors	Speed	Omissions	Errors
1	145	5.12	.17	117	.67	.49
2	138	5.10	.15	119	1.10	.45
Average	141.5	5.11	.16	118	.89	.47

spected per minute in the two periods in which the task was the same. These results show beyond a doubt that under the conditions of this experiment it is easier to determine or perceive the identity than the non-identity of the two letters in each pair, or at least that the former process takes place more quickly. There were no individual exceptions to this rule. With reference to the dispute whether judgments of similarity or of difference are more easily made, we can say at least that identity is more quickly perceived than the degree of difference existing between the letters used in this experiment.

It is rather difficult to offer a satisfactory explanation of these results. If, or whenever, there is a tendency to look at one of the letters of the pair a fraction of a second before the other, the explanation would be essentially the same as for the results of the other experiments. However, I am by no means sure that there is any such tendency. It may be that this is just one of the two or more factors all tending to produce the same result—if so, there would doubtless be individual differences in the relative importance of these different factors. Introspections might then be expected to differ.

Nearly all of the subjects stated that when they were canceling letters which were the same these pairs seemed to stand out more clearly than the others. Others went further and

stated that the main factor seemed to be a similarity or symmetry of outline, and that they hardly noticed what the letters were. When canceling pairs of letters that were different, some stated that they looked for these pairs, while others stated that they seemed to look for the pairs of identical letters, canceling those pairs which did not meet the requirements. Whatever else may be involved, we are probably safe in assuming that there is a greater or more wide-spread and complex cortical activity involved in the perception of "AK" than of "AA."

The results of this experiment also agree with those of Experiment II, in that there are more omissions but fewer wrong cancellations under positive instructions. These differences are in themselves conclusive evidence of a real psychological difference between the two working conditions, whatever they may be called. They also point to a similarity between the mental processes in these two experiments.

Summary

1. In the first experiment colored cards were shown, the subjects being instructed to move a lever in one way if the color were of a certain kind, in another way for any other color. The positive reaction times were, on the average, shorter than the negative.

2. It is easier to cancel groups of letters which contain certain letters than it is to cancel groups which do not contain these letters. 40 percent more work is done under positive than under negative instruction. There are fewer omissions but more wrong cancellations under negative instruction.

3. It does not require so long a time to determine the correctness of the answer in a multiplication equation as it does to determine the incorrectness of the answer. The average difference in time for the problems used was 145 sigma.

4. It is easier to pick out pairs of identical letters than pairs of different letters, where these two sorts of pairs are mixed together on a page. This difference is represented by the ratio 142:118.

5. These results indicate that the statement frequently made, that all negation is affirmation, is not true from the standpoint of the psychological and neurological processes involved.

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COMPARATIVE COGNITIVE REACTION-TIME WITH LIGHTS OF DIFFERENT SPECTRAL CHARACTER AND AT DIFFERENT INTENSITIES OF ILLUMINATION

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Introduction

Previous to the beginning of the present century all light used for purposes of general illumination was produced from incandescent solids, and hence possessed a continuous spectrum, of the same order as the solar spectrum, but differing in that it contained relatively less energy in the shorter wave-lengths, and more in the longer. The effect of monochromatic light, or of light possessing a line spectrum, on cognitive reaction-time was therefore a problem of pure science, and does not appear to have been investigated. (For the sake of brevity, the term "reaction-time" will be used henceforth, the words "visual cognitive" being understood.)

Early in this century Peter Cooper Hewitt discovered and developed a practical method of producing light from incandescent mercury vapor. Light so produced possesses the characteristic line-spectrum of mercury, in which a band in the greenish-yellow largely predominates, rendering it approximately monochromatic. As this light-source has come into general use for industrial purposes, the question of the effect of such light upon reaction-time becomes a matter of interest to applied, as well as to pure science. The present investigation was undertaken with a view to ascertaining what differences, if any, in such reaction-time would result from the use of light from mercury vapor, incandescent tungsten, and the sun under average conditions of daylight.

Spectral Character of the Different Kinds of Light Used

Mercury vapor light differs widely from the other two, its spectrum consisting essentially of four lines, *viz.*, a strong pair of greenish-yellow wave-lengths 579 $\mu\mu$ and 577 $\mu\mu$, an extremely brilliant green line of wave-length 546 $\mu\mu$, and a brilliant deep-

blue line of wave-length 436 $\mu\mu$. In addition to these there is a very faint crimson line, a faint blue-green line, and a pair of weak lines in the far violet. According to Bell's measurements,[1] about 90% of the visible energy of mercury vapor light is due to the three bands close together in the greenish-yellow and green, the balance being due to the blue line. This combination gives the light its peculiar "peacock blue" color.

Light from incandescent tungsten possesses a continuous spectrum somewhat richer in the red, orange and yellow, and weaker in the blue and violet, than sunlight. The curve, Fig. 1,

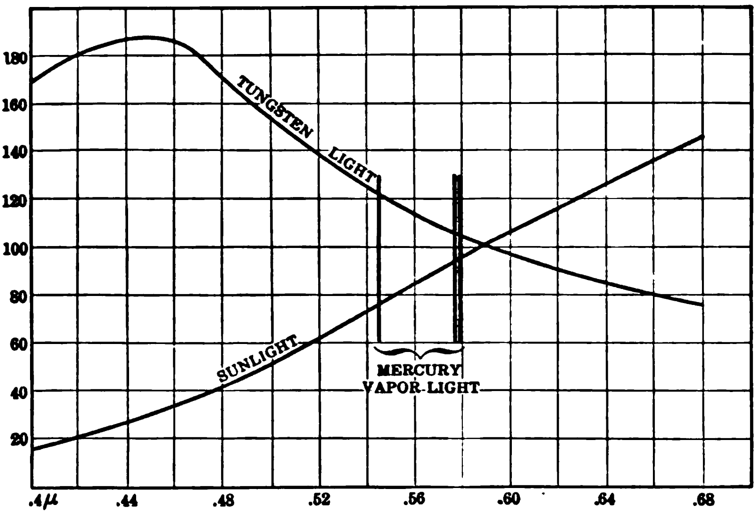


FIG. 1

shows the relative distribution of radiant energy in the spectra of blue sky, and the gas-filled tungsten lamp, equated to equal total visual intensity, *i. e.*, with 590$\mu\mu$ taken as 100 for each light, according to Luckiesh[2]. The vertical lines show the position of the three principal bands of the mercury spectrum.

Chromatic Aberration and Visual Acuity

In the early part of the nineteenth century Wollaston[3] showed that the refractive system of the eye is not achromatic, as had previously been assumed, but is slightly dispersive. Bell[4] states that "it is only in virtue of a very high maximum point in the luminosity curve of the eye that we are able to see distinctly at all. And it is this same fact which prevents the chromatic aberration being conspicuous under ordinary circumstances." This matter has recently been very carefully investigated by

Hartridge.[5] He finds that the relative dispersion of the refractive system of the eye is 45, which corresponds to that of a light flint glass.

The fact that the eye is subject to chromatic aberration naturally suggests the supposition that monochromatic light, by eliminating this optical defect, may increase visual acuity, or resolving power. This hypothesis has been investigated by Uhthoff[6], König[7], Orum[8], Broca and Laport[9], Ashe[10], Dow[11], Bell[12], Luckeish[13] and Rice[14]. There is a wide disparity in the conclusions reached by these investigators, which evidently arises from the difference in the method and apparatus used. Bell and Luckiesh used the same general methods, which consisted essentially in determining the relative intensities of illumination, measured on an equality of brightness scale, of light of different spectral character necessary to enable the eye to resolve certain test objects. Their conclusions are in very close accord, and show that the ratio of intensity of illumination by incandescent electric light to that of mercury vapor light, to produce equal acuity, or resolving power, is as 1.75 to 1, at ordinary intensities (1.5 to 2 foot-candles). Since it is well known that acuity is a function of illumination intensity, or surface brightness of the object, these results establish the fact that monochromatic light, or light of simple spectral character, gives greater acuity of vision than continuous spectra light.

Objects of the Present Investigation

Visual acuity being a function of the spectral character of the light, the further question suggests itself, whether reaction-time may not also vary with the nature of the light. The present investigation seeks to solve two related problems, *viz.*,

(1) What is the effect of light of different spectral character upon cognitive reaction-time?

(2) What is the effect upon cognitive reaction-time of different intensities of illumination?

As before suggested, these problems have a direct bearing upon manual labor performed under the direction of vision, since all such labor consists of muscular reactions in response to visual impressions in which cognition plays an important part. In devising the experiments, therefore, care was taken to insure actual cognition at each observation, and to eliminate the elements of visual resolution, practice, and fatigue.

Nature of the Experiments

The experiments consisted in exposing a test-object through an opening in the shutter, illuminated under given conditions, and requiring the observer to register his cognition of the object by pressing an electric key. The time required for the cognition and muscular reaction was recorded by a chronograph.

Apparatus

A special apparatus was constructed for carrying out the experiments. This consisted of a board 10x15 in., through the center of which was an opening 3/8x7/8 in., the whole being covered with neutral gray paper, and supported at an angle of 45° upon a baseboard. A shutter of gray cardboard, in which there was an opening slightly smaller than the opening in the board, was attached by a pivot to the underside in such a manner that the opening would register with the opening in the board. This shutter was actuated by a spring, and its motion controlled by two stops operated by electromagnets connected in series with two electric keys located on diagonally opposite corners of the baseboard. Each of these keys was also in series with a pen on the chronograph.

The test-objects consisted of numbers containing 4 digits, printed in 10-point Roman type on white bond paper in black carbon ink. 120 different numbers were printed on a tape of paper, and means were provided for passing this tape immediately back of the shutter, so that when the two openings registered one of the numbers was exposed to view. A duplicate set of numbers was also typed upon one side of the tape, which did not appear to the observer, but which registered with an opening visible only to the operator, thus showing to the operator the number presented to the observer.

A Gaertner chronograph was used, so geared that 1 sec. of time represented a distance of slightly more than 1 in. on the record. Two pens were provided, one of which was in series with the shutter magnets, as described, and the other in series with a clock pendulum which made electric contact through a mercury globule at each oscillation. The rate of vibration of the pendulum was determined by taking a chronograph record of its oscillations for 10 min., timed by a stop-watch, and dividing the total time by the number of recorded oscillations. The electric current for operating the electromagnets was supplied by 4 dry cells.

Sources of Light

The light used to illuminate the test-object was from three different sources; *viz.*,

A Cooper Hewitt mercury vapor lamp, Type F, operated on alternating current, 60 cycles, at 4 amps, and 100 volts at the terminals, the line voltage being reduced somewhat by the resistance used to maintain constant current.

Two tungsten filament, gas filled, incandescent electric lamps, of 200 watts and 300 watts rated capacity respectively, at 105 volts. The lamp-bulbs were fitted with "cap diffusers," *i. e.*, hemispherical specular reflectors which covered the lower part of the bulb, and projected the reflected light on to the white enameled still reflector, known in the trade as the "R. L.

M.," which was used with the lamps. The Cooper Hewitt lamp was equipped with the standard white enameled reflector supplied by the manufacturers.

Diffused sunlight admitted through one or two windows.

The wattage of the lamps was kept constant by the use of a rheostat, which was adjusted in accordance with the indications of a voltmeter in the case of the tungsten lamp, and an ammeter for the Cooper Hewitt lamp.

The room used for the experiments was 16 ft. square, with 10 ft. 6 in. ceiling, and had windows in the center of two adjacent sides, facing east and south, extending 10 in. from the ceiling. Each of the other sides contained a door. Walls and ceiling were painted light buff; floor Indian red; woodwork dark oak.

The apparatus was stationed on a small table placed slightly to one side of the center of the room, so that the electric light sources, which were suspended from the center of the ceiling, were entirely out of view of the observer, while shedding their light over his shoulder onto the test object. The apparatus was placed diagonally to the room, so that light from either window struck the test object at approximately the same angle as the artificial light, and so as to avoid any direct reflection from the observer's eye.

Adjusting and Measuring the Intensities of Illumination

The intensity of illumination on the test-object was regulated by lowering the lamps slightly for the higher intensities, and screening off a portion of their light for the lower intensities, in the case of the artificial light; and by adjusting the opaque shades of the windows by raising them from the bottom, in the case of sunlight.

The intensity of illumination on the test-object was measured with a Macbeth Illuminometer, which is a portable, equality-of-brightness photometer, using a miniature tungsten lamp run from a small storage cell as the standard light-source, a Lummer-Brodhun screen, and producing a balance by moving the standard light. In measuring illumination a disk of opal glass, having the surface sand-blasted, is placed on the given surface, and the brightness of the plaque balanced against the standard light. In measuring the illumination from mercury vapor light and sunlight, ray-filters were used to bring the color of the standard light to match that of the light measured. The instrument had been recently calibrated by the Electrical Testing Laboratories, New York City, a flicker photometer being used in determining the absorption coefficients of the ray-filters used in measuring mercury vapor light, and sunlight. Ten readings were taken of each intensity with each light, after the final adjustment, and the result was checked by an equal numbr of readings taken at the close of the series of observations.

Method of Carrying out the Experiments

The observer was seated in an arm-chair in front of the special apparatus stationed on the table. By slightly inclining the head the opening in which the test-object appeared was in the direct line of vision, the plane of the paper tape being substantially normal to the visual axis. The observer was instructed to adjust the distance from his eyes to the object for the clearest vision, which was done unconsciously after he became accustomed to the work. The electric key with which he registered his reaction was in the position at which his right hand would naturally rest when his arm was supported on the arm of the chair. It required a force of 200 gms. exerted through a distance 0.5 cm. to make contact.

The operator was seated on the opposite side of the table, with the key which controlled the first motion of the shutter within convenient reach, and the opening in which the duplicate numbers on the tape appeared in plain sight. The chronograph and clock were on a table at the side of the room, convenient to the operator, and connected to the apparatus electrically by a piece of flexible bell-wire. The voltmeter and ammeter were on the apparatus table, to which the rheostat was also attached, all within easy reach of the operator. The paper tape was passed under the shutter by being rolled from one drum to another by means of hand wheels at the side of the apparatus, convenient to the operator's left hand, and the shutter was set by being pushed back to its original position, in which it was held by a latch.

The illumination having been adjusted according to the prescribed conditions, the chronograph and clock were set in motion, with both pens in contact with the record paper on the cylinder. The observer was instructed that a succession of printed numbers would appear in the opening in the center of the board, and that as soon as he identified a number he was to press the key at his right hand, afterward repeating the number, so that the operator could check it against the duplicate number appearing at the back of the apparatus, thus making sure that it had been actually identified. The front-board of the apparatus covered the larger part of the visual field of the observer during his observations, thus insuring uniformity of contrast with the test-object, and entire absence of glare. All in readiness, the operator pressed the key, the shutter moved into its position with the opening disclosing a number on the tape; the observer identified the number, pressed the key which released the shutter so that it eclipsed the number, and repeated the number observed. The operator then reset the shutter, turned the tape-drums sufficiently to bring another number into

position, and the observer repeated his operations. This was continued until all the numbers on the tape had been identified by the observer.

Observers

In all, 5 observers were used. The first experiments were made with the 3 different lights, at a uniform intensity of 5 foot-candles, with all 5 observers. These initial experiments were considered rather as qualitative, to ascertain if any appreciable differences in reaction-times would appear, and if so, with what consistency of results. The results proving entirely satisfactory on this point, two of the observers were dropped in the remaining experiments, and their observations at this intensity were omitted from the final calculations. The results of their observations showed the same consistency in their averages and mean variations as those of the other observers in subsequent work.

The 3 observers used were all men, of various ages and vocations as follows: B, age 34, accountant; J, age 29, stenographer; O, age 54, salesman. All wore glasses during the observations.

Method of Measuring the Reaction-Time

The elapsed time between the presentation of the stimulus and the completion of the reaction is represented by the distance between two successive lateral movements of the pen on the chronograph record. The sum of 100 of these distances, representing a set of 120 observations less the first 20, was obtained by adding the individual distances mechanically by means of a a pair of dividers, the successive spaces being laid off along a line on a tape of paper.

The linear distance corresponding to 1 sec. of time was determined by taking the length of a line on the record traced during 10 min., as indicated by a stop-watch, and dividing it by 600. This distance (slightly over 1 in.) was used as the unit for making a scale by which the reaction-times, in the form of linear distances, were measured. The length of the line representing 100 observations, measured by this scale, thus gave the average reaction-time in sec. when divided by 100.

Method of Computing the Mean Variations

An apparatus was constructed, which consisted of a board 30 in. long and 4.5 in. wide, supported at an angle of 45° on a baseboard. A ledge was provided running lengthwise along the center of the board, and a piece of adding machine tape (white paper 2.25 in. wide) was stretched along the upper half, resting against the ledge, upon which was laid the scale of seconds. A slide having its edges perpendicular to the ledge was provided,

which could be moved along over the paper. A slat was attached to the front edge of the baseboard, which was beveled to 45°, which served to hold the chronograph records in place when passed under it, and afforded an accurate guide for the divider points in taking off the distances. A pair of proportional dividers was used, the short and long legs having a ratio of 1.4.

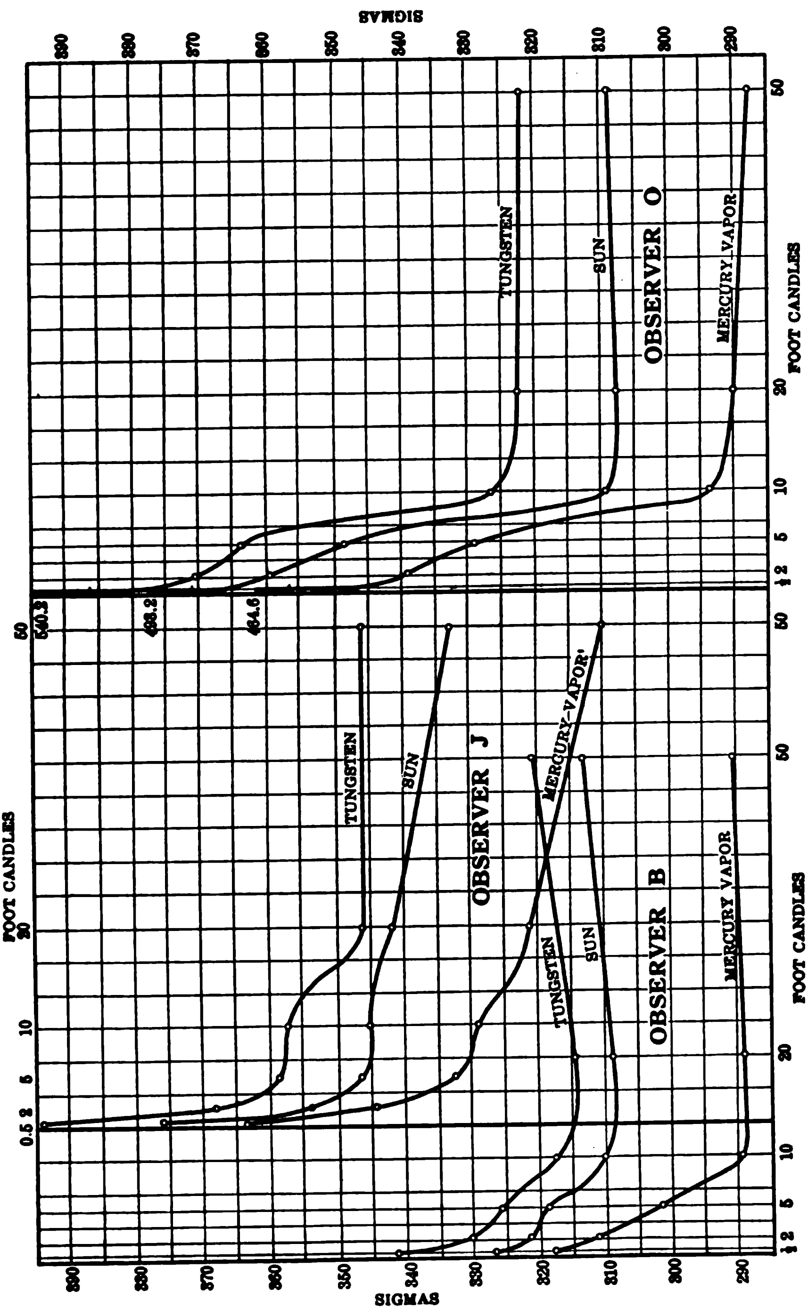
Setting the edge of the slide at the o mark on the left of the scale, the distance representing one reaction-time on the chronograph record was taken off with the short legs of the dividers, and pricked off along the lower edge of the tape, the slide being moved along the distance thus laid off. The distance between the points of the long legs of the dividers was then pricked off along the edge of the slide, perpendicular to the length of the tape. The second reaction-time was then taken off, added to the first in the usual manner, the slide moved up, and the vertical distance laid off against the slide as before; and so on for 100 observations. The total time was then measured on the scale of seconds, and the result, when pointed off, gave the average time in sigma.

Setting the short legs of the dividers to a distance representing this average time, the distance between the points of the long legs was used to set the o point on a small movable scale attached to the edge of the slide. The distance of any given point laid off along the slide from this mean value corresponding to the o mark on this scale represented the variation of that reaction-time from the mean. The sum of these variations was obtained by moving the slide the length of the tape, reading the distances of the points in like manner, and adding the scale readings. The total thus obtained was a linear quantity, and was reduced by the dividers, measured by the seconds scale, and pointed off for sigma, which gave the mean variation for the set of observations.

Number and Arrangement of Experiments

Six series of experiments were made at intensities of 5, 10, 20, 50, 2, and 0.5 foot-candles, in the order given. In each series results were obtained by 3 observers under the light of tungsten, mercury vapor and the sun. At 5 foot-candles 2 observers took 500 observations under each light; one took 300. At 10 foot-candles all 3 observers took 500 observations; at 20 foot-candles all 3 took 300; at 50 foot-candles all 3 took 300; at 2 foot-candles all 3 took 500, and likewise at 0.5 foot-candle.

Each series of experiments consisted of 120 observations made under each of the three kinds of light at the same intensity, by a single observer at a given sitting. The order in which the different lights were used was rotated in the different series. Practice series were taken until the m. v. of any set of observations was 10% of the average, or less.



CHARTS I, II, III

TABLE I

Visual Cognition Times under 3 types of illumination, at 6 intensities. Averages and mean variations

	Series 6 ½ foot-candle		Series 5 2 foot-candles		Series 1 5 foot-candles		Series 2 10 foot-candles		Series 3 20 foot-candles		Series 4 50 foot-candles	
	AVERAGE	MV	AVERAGE	MV	AVERAGE	MV	AVERAGE	MV	AVERAGE	MV	AVERAGE	MV
OBSERVER B												
Hg	318.4	31.42	311.2	25.98	301.8	21.30	289.8	18.00	289.3	17.80	290.3	18.90
S	326.8	35.02	321.6	24.62	318.5	23.50	309.8	19.60	309.0	19.30	313.3	23.70
T	340.8	36.94	334.8	27.28	330.6	22.50	316.2	21.10	314.6	22.20	320.6	24.10
OBSERVER J												
Hg	363.6	30.96	344.8	27.84	332.3	20.93	329.0	22.10	321.3	18.80	310.0	18.40
S	375.2	30.82	354.4	27.46	346.0	22.60	345.0	25.06	341.6	19.00	333.3	18.30
T	394.0	36.14	367.6	29.12	358.7	24.40	356.2	26.30	345.6	19.10	345.6	18.30
OBSERVER O												
Hg	464.6	56.74	338.4	27.50	327.2	25.60	293.6	20.50	290.0	16.80	288.0	17.40
S	493.2	71.42	359.0	32.30	347.5	29.62	309.2	20.60	307.6	18.40	309.3	19.30
T	540.2	65.12	369.2	31.90	362.2	29.14	326.0	25.20	322.3	25.10	321.6	20.20

In order to afford a "warming up" process before each set of observations, 120 reactions were taken, of which the first 20 were omitted in the calculations. The different series of experiments using different intensities of illumination were taken at widely separated times, extending over a period of 9 months.

Results

The general averages and the mean variations of the cognition times, under the 3 lights at the 6 different intensities, are given in Table I. The curves of the general averages of the 3 lights at the 6 intensities are given in charts I, II, and III, the illumination in foot-candles being plotted as abscissae, against time in sigma as ordinates.

TABLE II

The Difference, the Probable Error of the Difference,[16] and the Probable Correctness of the Difference,[17] between the Average Reactions under Mercury Vapor Light and Sunlight at 6 Intensities of Illumination

Intensities of Illumination	Observer B Dif.	P. E.	P. C.	Observer J Dif.	P. E.	P. C.	Observer O Dif.	P. E.	P. C.
½ foot-candle	8.4	1.777	99.90	11.6	1.620	100.00	38.6	3.470	100.00
2	10.4	1.356	100.00	9.6	1.484	100.00	20.6	1.605	100.00
5	16.7	1.202	100.00	13.7	1.502	100.00	20.3	1.513	100.00
10	20.0	1.013	100.00	16.0	1.263	100.00	25.6	1.098	100.00
20	19.7	1.283	100.00	20.3	1.304	100.00	17.6	1.215	100.00
50	23.0	1.479	100.00	23.3	1.266	100.00	21.3	1.270	100.00

TABLE III

The Difference, the Probable Error of the Difference, and the Probable Correctness of the Difference, between the Average Reactions under Mercury Vapor Light and Incandescent (Tungsten) Light, at 6 Intensities of Illumination

Intensities of Illumination	Observer B Dif.	P. E.	P. C.	Observer J Dif.	P. E.	P. C.	Observer O Dif.	P. E.	P. C.
½ foot-candle	22.4	1.832	100.00	30.4	1.769	100.00	75.6	3.266	100.00
2	23.6	1.428	100.00	22.8	1.524	100.00	30.8	1.593	100.00
5	28.8	1.171	100.00	26.4	1.567	100.00	35.0	1.431	100.00
10	26.4	1.052	100.00	27.2	1.298	100.00	32.4	1.227	100.00
20	25.3	1.390	100.00	24.3	1.307	100.00	32.3	1.474	100.00
50	30.3	1.499	100.00	35.6	1.266	100.00	33.6	1.303	100.00

TABLE IV

Ratios of the Cognition-Times under 3 Types of Illumination at 6 Intensities.

Intensities of Illumination	B Hg	B S	B T	J Hg	J S	J T	O Hg	O S	O T
½ foot-candle	1.000	1.226	1.070	1.000	1.033	1.083	1.000	1.061	1.162
2	1.000	1.033	1.075	1.000	1.027	1.066	1.000	1.060	1.091
5	1.000	1.055	1.095	1.000	1.041	1.079	1.000	1.062	1.107
10	1.000	1.069	1.091	1.000	1.048	1.082	1.000	1.053	1.112
20	1.000	1.068	1.087	1.000	1.063	1.075	1.000	1.060	1.111
50	1.000	1.078	1.104	1.000	1.075	1.114	1.000	1.073	1.116

The reactions taken under mercury vapor light are shorter than those under sunlight, and those taken under sunlight are shorter than those under tungsten light. This relation is constant for all 3 observers at all 6 intensities, though the amount of difference as shown in Tables II and III varies with the light and with the intensity. It will also be seen that, in general, the reactions decrease in length as the illumination increases in intensity; this relation is true for all observers and all lights to an intensity of 20 foot-candles. From 20 to 50 foot-candles under tungsten light the reaction-time for one observer increases, one remains the same, and with one it decreases very slightly. Under sunlight the reaction-time of two observers increases; with one it decreases. Under mercury vapor light the reaction-time of one observer increases very slightly; with two it decreases.

Tables II and III show the difference between the averages of the cognition-times taken under mercury vapor light and sunlight and the difference between the average under mercury vapor light and tungsten, respectively, at the 6 intensities of illumination used, also the probable error of the differences, and the probable correctness of the differences between the sets of cognition-times. The probable errors are small (except in the case of observer O at 0.5 foot-candle, where the m. v.'s were exceptionally large, as were also the differences). The probable correctnesses are either a mathematical certainty, or approach the limit.

Table IV shows the ratio of the differences in time under the 3 lights at each intensity. These differences are smallest at the lowest intensity, and in general tend to increase with the intensity.

Sources of Error

Instrumental Errors include the following:

(1) Variation in rate of revolution of chronograph cylinder
(2) Error in linear distance representing 1 sec. of time
(3) Error in scale of sec. by which reaction-times were measured
(4) Variation in speed of shutter

Accidental Errors include:

(5) Errors in setting the divider points in taking linear distances
(6) Errors in reading the variations in reaction-time on the scale
(7) Errors in making photometer readings

Psychological Errors could arise from the following:

(8) Fatigue
(9) Visual adaptation
(10) Prejudice
(11) Physiological indispositions
(12) Practice
(13) Distractions

These sources of error will now be considered in detail.

(1) One revolution of the cylinder recorded 28 oscillations of the pendulum, which were shown on all records as taken as a check on the regularity of motion of the chronograph. Measurements of the distance between these 28 beats on all records taken showed a mean variation of 0.24 of 1%, and a probable error of .01 of 1%. Applied to the maximum and minimum average times this would give probable errors of .054σ and .028σ respectively.

(2) The results of 3 measurements of the number of pendulum beats recorded in 10 min. gave a m. v. of 0.21 of 1%, and a probable error of 0.625 in the unit of length representing 1 sec. on the scale. As before stated, this being a constant error, and therefore not affecting the differences, no special effort was made to reduce it.

(3) This is subject to practically the same conditions as (5): but the errors were constant as affecting the final results.

(4) This was not measured, as it was sufficiently rapid to produce instantaneous appearance of the stimulus, so far as visual perception is concerned.

(5) The error of a single setting of the dividers was within 5%. The reduction of this error by the canceling out of + and — errors in the 100 settings must bring it near the 0 point on the probability curve; so that the error in the average time was well within the limits of instrumental error.

(6) The same general conditions apply as in (5).

(7) The m. v. in reading photometer settings was 5%. Since the variation in reaction-time due to difference in intensity of illumination was relatively small, the errors in photometric readings are negligible.

In general, it would appear that the mechanical and personal errors of measurement, at least the variable errors which affect the significance of the results, are no greater than—probably not as great as—would have resulted from the methods and apparatus more commonly used for such experiments. There is no doubt that the labor of calculation was much less, and practically free from clerical error.

(8) Care was taken to avoid fatigue by giving rest-periods of 5 to 7 min. between the sets of observations, and two periods of two min. each during the observations. A comparison of the first 10 observations with the last 10 in a representative number of cases showed no discrepancies in average reaction-time, thus proving the absence of fatigue.

(9) The observers were in the room under the adjusted illumination from 5 to 10 min. before beginning their observa-

...gher intensities, and longer at the ... observations were omitted in the ... for adaptation and practice. The ... beginning and end of sets of observa... applies equally in this case.

... one kind of light might affect results; ... would necessarily be very marked, ... appear as wide and erratic differences ... The fact that no such fluctuations ap... ...trary the recorded times show a remark... ...ficient evidence that they were not subject ... prejudice.

... were all in apparently normal health and ... the observations. Furthermore, each ob... ...tions on 5 different days for each intensity, ... test, for which they made observations on 3 ... he observations at the different intensities ... period of 9 months. The total results therefore ... which eliminates the factor of physiological

... ods used to eliminate errors of practice have ... scribed.

... of mechanical impact in the apparatus were ... felt, so that it operated without sensible ... The room had no other occupants than the ... server during the observations. There was no ... of vision of the observer, either while he was ... vations or during his rest-periods. The ticking ... and the slight clicks of the chronograph mag... ... sounds in the room. The noises without were ... vers were accustomed to hear regularly in their

Discussion of Results

... would appear a difference in reaction-time between ... vapor light and continuous spectra light was rather to ... in view of the fact that monochromatic light in... ... acuity. It has been generally held, at least among ... engineers, that this gain in acuity was compar... ... in evidence at the higher intensities, but became ... the lower intensities. It will therefore be a surprise ... of the applied science of light to learn that ... reaction-time between mercury vapor light and ... light increases with the intensity of ... two-thirds of the cases, which would seem to ... It will probably also be somewhat surprising

to learn that there is no appreciable difference in reaction-time between daylight and tungsten light under the same conditions.

Why these differences should exist is a matter for speculation rather than explanation. Until "the enigma of color vision," as Troland aptly puts it, is solved, an ultimate explanation is apparently impossible. We know, however, that vision by white or polychromatic light is a complex process, and that the visual impression received is the resultant of some kind of summation-process taking place either in the retina or at the cortical centers; and it is conceivable that this process requires time for its consummation. Vision by monochromatic light, not being the resultant of a complex stimulus, may therefore require less time. Purely nerve functions can hardly have any part in the phenomena.

The difference in time between daylight and tungsten light is less suggestive of explanation. There are three factors which may enter into the problem: the difference in color of the two lights will produce a slight variation in the contrast between the test object and the field (the figures and the paper); the two lights exert a different amount of energy for a given visible intensity; and the fact that the eye has been developed under daylight conditions may cause it to function more rapidly under such light than under a light differing in color composition. Which of these causes is operative, or whether all have a share in the result, will require special investigation to determine.

That reaction-time diminishes as the intensity of the stimulus increases is a well established fact, with which the results in the present case agree. The intensity of illumination necessary for minimum reaction-time under any given light does not appear to have been quantitatively determined heretofore. Rice proposed to determine the intensity of illumination of continuous spectra light (daylight and incandescent light) required for the "most efficient vision," by which he meant the greatest visual acuity. He states that, of the various uses of artificial light, the most important is that for reading. In view of the use of the artificial light in the industries, this contention can hardly be maintained at the present time. The fact that the best daylight conditions can be fully equalled by artificial light for the performance of manual labor is of such far-reaching importance as to be classed among the revolutionary improvements in industry, comparable with the use of the steam engine for motive power.

Cognitive reaction-time affords the only parallel to actual working conditions where the hand is directed by the eye; and the cases to which this does not apply, at least to a considerable extent, are comparatively few. The intensity of illumination required for minimum reaction-time is therefore the most reliable

guide for determining the illumination intensities required in industrial lighting.

From the results obtained in these experiments it appears that 10 foot-candles is the practical minimum required where sharp visual focussing is necessary, and the best conditions as to contrast in the objects seen, and sources of external disturbance, such as glare, etc., prevail. Rice concludes that "intensities of 8 and 40 meter-candles (practically 1 and 4 foot-candles) constitute approximately the lower and upper limits respectively of suitable illumination for ordinary purposes." These are much too low for industrial purposes, as the present experiments show, and as recent observations under practical factory conditions corroborate.

Conclusions

(1) Continuous spectra light produces a lag in cognitive reaction-time as comparable with the line spectrum light of mercury vapor of equal intensity.

(2) The light from incandescent tungsten produces a sensible lag in reaction-time as compared with normal white light (daylight, or diffused sunlight) of equal intensity.

(3) The differences in reaction-time, in general, vary directly with the intensity of illumination.

(4) The minimum intensity of illumination required for maximum visual efficiency in reaction-time is between 10 foot-candles and 20 foot-candles.

The writer wishes to express her thanks and obligations to Prof. H. P. Weld, Department of Psychology, Cornell University, for valuable advice and assistance in planning the experiments.

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[15]Computed by the formulae:

$$\text{P. E.} = \frac{0.8453 \text{ m. v.},}{\sqrt{n}} \text{ and P. E. diff. A-B} = \sqrt{(\text{P.E.}_A)^2 (\text{P.E.}_B)}$$

[16]Computed by the formula given by Boring. *Amer. Jour. of Psych.*, XXVII, 1916, 315-19.

THE MIRACLE MAN OF NEW ORLEANS

By John M. Fletcher, Tulane University of Louisiana

During the spring months of the year 1920 there came to New Orleans an old man presenting the typical mien and make-up of a latter day prophet, who has made this city a rival of Quebec as a center for miraculous healing. It is reported that in 1903 the pilgrimages to the shrine of St. Anne de Beaupré, near Quebec, including persons who had been healed and those who were seeking to be healed, amounted to 168,000. No account has been kept of those who have visited the New Orleans Miracle Man, but if one were to include those who came merely to see what was being done it is very likely that the number would exceed the total of those who visited the famous Quebec shrine in 1903. It is interesting to note that the New Orleans Miracle Man is also of Canada, having been born there in 1847. In the veins of many whom he attempted to heal runs the blood of the exiled Acadians, who make up a considerable percentage of the population in certain regions of Louisiana. The 'Cajans', as they are known locally, include a large percentage of illiteracy and also a large percentage of those who cannot speak English. It would not be true to say, however, that only among the 'Cajans' was the faith in miraculous cures able to secure a foothold. There have been many persons of all nationalities, and representing all sections of the country and all strata of society, who have become converted to the belief in the old man's claims. A reporter on one of the city papers, who has recently come from New York, and who says his father was a physician, took the writer to task for asserting that there had been no authenticated case of the cure of an organic disease by the healer. Yet on the whole, as in all such cases, the great background on which the entire movement rests is one of ignorance and superstition. One does not have to do more than to visit one of the open-air demonstrations, and watch the types of faces uplifted in hope and the hands outstretched in pathetic appeal, to realize that this is true.

It has seemed to the writer that this case holds something that is of interest to psychology and that it should therefore be noticed and recorded. The social aspects of the case are now being investigated. Local psychiatrists are also seeking the

opportunity to make a study of it. The following account is given with a view to presenting merely the facts and general impressions of the event itself. This account contains the substance of a paper read before the Southern Society of Psychology and Philosophy at its annual meeting at Tulane University in April, 1920.

As to the Miracle Man himself it must be said that very little is known and very little can be found out. His real name is John Cudney, though on the occasion of his christening an older sister, who is reported by him to have had "foresight," said he was destined to be a prophet and wished him to be called Isaiah. In the family he was called Brother Isaiah, and it is by this name that he still prefers to be known. The circumstance of his christening in this fashion and the force of the suggestion in his name seem to have had much to do with determining his career.

In his early life he made, so far as has been ascertained, no attempts to effect cures, or to exercise any other unusual gifts.

From Canada he came to America some time in his youth. As a young man, while walking alone in the woods of Nebraska, he claims to have heard a divine voice telling him that he was called upon to heal people of diseases. This seems to have meant to him that he should desert his family. He relates that he agonized with God over this matter far into the night, but arrived at no solution. The following morning his wife, who seems also to have had the same revelation about him, announced that she and their sons would have to give him up so that he could devote the rest of his life to the work to which he had been called. Since that time he has traveled in many parts of the world healing by prayer and, like the apostles of Christ, earning his living meanwhile. He has apparently operated in many other American cities, although not so conspicuously as in New Orleans. In certain instances he seems to have been "invited" to leave by the city authorities.

In spite of his popular title of the "Miracle Man," he does not lay claim to performing miracles. He says that the power to heal diseases comes to him periodically, but that all he does is done through the goodness of God. He disclaims being a Christian Scientist, though like the members of that communion he believes that God does not will disease. The truth of the matter seems to be that he is probably incapable of working out any consistent notion of what he proposes to do. He uses an oil of wintergreen to rub those whom he treats, but he says that this has no curative properties, and is merely to decrease friction. He makes use of magic by blessing handkerchiefs and sending them to patients who are not able to reach him. In watching his healing one day the writer heard him speak of epilepsy as demonic possession. He said, "I have had a great

deal of experience with them cases. And I tell you when them epilektic fits come out they makes a lot of noise." He presents all the appearances of being a devout, simple-minded religious fanatic. He could easily have made a Peter the Hermit or a St. Simeon Stylites. Those who know him privately and intimately speak well of his character and absolve him from conscious fakery. Whether he can survive the notoriety thrust upon him remains to be seen. He has been repeatedly offered money, but either from fear of the law or from principle he seems to have refused it, though he does accept gifts. There are rumors that he has received money, though these are difficult to prove. Temptations of a sex character are also assailing him. Certain of his female 'cures' do not hesitate to kiss him and fondle him in public. This, coupled with the fact that he preaches that a wife should leave a husband if God calls her, makes it entirely possible that the matter may at any time have a sudden and unsavory ending.

The story of how he came to attract such extraordinary attention not only illustrates the human craving for the supernatural but at the same time indicates the responsibility of the public press, a responsibility which is not always fully appreciated. For several days the papers of New Orleans debated among themselves whether they should give publicity to what was being done. They presently decided in the affirmative; and about the first warning that the public had was the burst into print of accounts of wonderful cures effected by a strange old man in a little house-boat on the mud banks of the levee at the foot of Calhoun Street. These accounts produced a marked impression on the entire city. Everywhere on the streets and in the homes people were talking about Brother Isaiah. Through the press dispatches reports went to other cities. Moving-picture concerns seized upon the new sensation and scattered the distorted rumors still further. Even the billboards of Broadway, New York, gave space to this thriller.

It seems that the healer had been at work for some time prior to his burst into notoriety. He had in fact been to New Orleans once or twice before and had made acquaintances along the river-front. His reputation began to grow in the earlier months of this year to such an extent that it became necessary to call extra police-help in order to disentangle the automobiles that came to his home. There was even at this stage a curious mixture of the poor and the rich among his patrons. Some hobbled on foot, others came in elegant limousines. The people who first went to see him were those interested in being healed or in having some member of the family treated. After the front-page account in the newspaper came out, the health-seekers were joined by a throng of the curious. Extra street-cars were

put on the lines leading to that portion of the river front. Great masses of pedestrians and people in automobiles crowded the levee daily. The number of persons seeking treatment assumed alarming proportions. The sick began to arrive on all trains, without taking the precaution to make inquiry about accommodations. The charitable organizations, the hospitals, the Red Cross, and the city officials found themselves with a problem on their hands. Letters, telegrams and long-distance telephone calls poured into the offices of the newspapers; they had apparently got more than they bargained for. Conditions of great distress began to spring up about the old man's place of operation. Invalids who perhaps had not left their rooms or beds for months came and stood for hours in the cold March wind and sometimes in the rain awaiting their turn. Patients who were almost delirious with fever would stand with their head on the shoulder of a relative in the long line of suffering. An occasional groan of pain would elicit the comforting statement that their turn would come soon. No toilet accommodations had been provided, much less any shelter or food. It became a problem of serious concern to the State Board of Health. It became necessary to protect the health of the community, and at the same time it seemed wise to avoid any appearance of persecution of those who were holding with mad fanaticism to the faith of the old healer. One can imagine how unsanitary the whole procedure was when told that he was rubbing and manipulating his patients one after the other all day and most of the night without even washing his hands. It was reported that leprosy had appeared among his patients. On account of these dangers it became necessary to remove the tents that had been set up by the Red Cross on the levee for the protection of those who had left their homes to come for the treatment.

In the height of the excitement of the early days of his recent popularity one could hear on all hands wonderful stories about what the 'miracle man' could do and had done. Some said he was Christ appearing on earth again. The story went around that he had once stopped a shower of rain by holding up his hand. The credulity and the will to believe upon the part of the well, and the desperate hope of the sick, made out of the situation a veritable rumor factory. An appreciation of the setting of this case seems to be necessary in order to get an idea of the atmosphere out of which these rumors grew. Miss Doris Kent, a former student of the writer and a graduate of Newcomb College, Tulane University, was assigned by the Times-Picayune of this city to write the matter up. She remained on the assignment until threatened with violence by one of the self-appointed managers, who was suspected of carrying on a petty graft-scheme by which he could for a consideration secure

prompt attention from the healer. Miss Kent thus describes the situation as she saw it March 13th:

"Steadily swelling crowds, excitement rising to white heat throughout the city and community, dozens of new 'cures' and a few bits of conflicting testimony were results of another day and night of 'faith healing' on the levee off Audubon Park, where John Cudney, or 'Brother Isaiah,' has worked steadily for three days and three nights, praying for the healing of the sick and the defective.

"His great frame sagging slightly with weariness, his face almost as white as his long hair and his snowy beard, the old riverman had hardly paused for rest or food since the first rush upon his little houseboat began Wednesday afternoon. As he prays over some twisted form on a little rudely-erected platform in the mud, hemmed in so closely by the crowd that scarcely a breath of air reaches him, he pauses for a moment to swallow a few mouthfuls of orange or pineapple juice, passed to him over the heads of the crowd. Back in the tiny houseboat, that was almost sunk Thursday, when the mob pressed aboard, Mrs. Coldberg, the 77 year old sister of the 'healer', prepared the only nourishment he found time to take."

The following is given as a picture of what the situation looked like March 14:

"Paeans of joy from men, women and children who professed to be cured in an instant by 'Brother Isaiah's' powers continued to go up from many sources Friday.

"Watch fires were built all along the levee and down on the river beach late Saturday night by those who were determined to obtain close-up positions when 'Brother Isaiah' resumed his practice, which it was said he would do early Sunday morning. The bivouac of the 'faithful' presented a weird appearance, and hundreds of sight-seers journeyed in automobiles to look on the strange scene. Carnival and the Day of Judgment combined best expresses the atmosphere on the Audubon Park levee Saturday, when 5000 persons at one time gathered to witness the 'faith healing' of John Cudney, the 'Brother Isaiah' who has thrown the city into a turmoil with his alleged 'cures'.

"By nightfall (of March 16) a village of little white tents had sprung up like a growth of mushrooms along the embankment. The American Red Cross has contributed ten tents and one hundred cots, and will provide more if necessary to house the unfortunates whose hope drives them to remaining at their posts day and night......................A large platform will be built for him later, since the 'healer' has refused all offers of a hall, declaring that he must do his work in the open air on the spot where he first began.

"Surroundings rapidly are becoming dangerously insanitary upon the levee. Since Sunday the spot has taken on the aspect of a lot just vacated by a circus. The ground is trampled bare for a long distance, and every vestige of grass has been wiped out by the thousands of feet. The waiting line stands at the foot of the levee toward the river, and in the hollow has collected a drift of tattered papers, rotting fruit, fragments of food, broken bottles, torn boxes,—all at the feet of the wretched ones who have stood for more than twenty-four hours packed between the ropes about the runway. In the sultry, humid atmosphere of Monday afternoon the place was repellent to every sense, yet the dreary line still stood with abject patience, scarcely speaking among themselves or noticing the reduced ranks of the sightseers who stood on the higher ground.

"Petty commerce thrives all around the outskirts of the crowd about the 'miracle man'. The peanut, popcorn, soft drink, and fruit wagons are

thoroughfare is an array of photo- … hung artistically upon the red

… cures it seems quite difficult … of truth which must as a … which such excited rumors … say that no organic diseases … his healer. Out of the vast … for treatment there must have … functional character, which … treatment he offered. But … cases they are difficult to … about what was said to … instances are not so easy to … the case is that the minds … substantiation. They were … reports without it. If one … surrounded the old man … would have to believe … disease, blindness, … other forms of human … numerous.

… to the wild rumors … who was born blind … restored. The … This was … succession that … case to see what the … many other cases that … basis of fact whatever … which subsequent inves- … cure had been effected. … what was called rheumatic … himself. He seems … cured up to date.

… cures were very numerous. … the same time a sample of … are afforded in the case … young man had been a … He made his way to … after much difficulty se- … much interested in Benny's … After the treatment a … intense curiosity. They … "No, he is running!" … able oaths to give vent … about him that it was

quite impossible for anyone to see what was going on, but from those who were near him it was subsequently found out that he had neither been running nor walking, but that he had been carried forward bodily by persons who had caught him under the arms. It was stated that it was doubtful whether his feet touched the ground at all during this exciting journey. Before he reached his home the rumor came back that he was in the same condition as before the treatment. A man is reported to have gone up for treatment of cross-eyes. The crowd, having forgotten what he was being treated for, and having taken him for a paralytic, shouted "another miracle" when he walked away. Another case of this character is that of an imbecile girl who was dumb. She was brought by her mother to be treated. While waiting on the outskirts of the crowd she began to mutter, doubtless in her usual fashion. The crowd took her to be a 'cure' and began to gather around her to hear her verbigerations. She naturally grew excited and talked the more vehemently. The mother strove in vain to tell the crowd that the child had not even seen the miracle man.

The extent to which the excitement and bewilderment penetrated the city is illustrated by the story of the man who had some time ago lost one eye. Without the knowledge of his wife he had a glass eye inserted. When he went home at night his wife asked in surprise what had happened to him. He replied that he had been treated by Brother Isaiah. Before he could control the situation his whole family fell on his neck and rejoiced.

The Chief of Police of New Orleans sent a test case about the middle of March in the person of Mr. John Mayes, formerly conductor on the Illinois Central Railroad. Mr. Mayes had suffered a stroke of paralysis about a year previously, which resulted in hemiplegia of his right side. His speech has also been interfered with, so that he is able to say only two words, 'no' which he repeats over and over, and 'Lee' the name by which he now designates his wife. For three days he had waited his turn for treatment. Both he and his wife had the utmost faith that the treatment would be successful. When his turn finally came he was carried onto the pier by the negro body-servant who is his constant attendant, and was placed in a chair in the presence of the healer. The account of the treatment of this case says:

"The afflicted man sat with his eyes glued to the face of the 'healer' while hope fairly blazed from them. Back of him his wife stood, with hands clenched tightly together, whispering encouragement. The 'miracle man', gaunt and weary, in his long blue garment, like the apron of a surgeon, bent over him with faith as fervent as the hope of the patient. Kneeling beside the chair, the big negro, his hands trembling with excitement, gently removed the overcoat and coat of the paralytic and held the little bottle of oil while the 'miracle man' rubbed the afflicted shoulder and forehead of the

[illegible] moments of prayer he suddenly looked into the eyes [illegible] "Say your name, say John!" The throat of the [illegible] and swelled with the effort; his eyes never left [illegible] man but the only sound that came forth was "No! [illegible] the healer. "Not no! say yes!" Painfully the man [illegible] but his eyes filled with tears as he failed again and [illegible]" the healer cried again. "Call upon the Lord, [illegible] the name that forced itself from the agonized lips of the [illegible] was the name of the wife behind him, who burst into tears [illegible] in his anxiety to help the man made a figure [illegible] two. He tried again and again, with prayer, [illegible] urgings, but at last the paralytic was carried [illegible] his servant with the promise of 'later treatment', [illegible] would be effective."

[illegible] cases did not daunt the courage of the healer [illegible] expectation of the believers.

[illegible] Lacoume is of interest. Lacoume is the [illegible] well-known blind newsboy-musician, who is reported [illegible] the first introducers of that world-renowned New [illegible] product, jazz music. When a newsboy on the streets [illegible] to have attracted the attention of Olga Nethersole [illegible] Sarah Bernhardt, each of whom desired to send him [illegible] educated in the schools for the blind. After being [illegible] the healer, Lacoume was told to go to his home and [illegible] eyes closed for 24 hours, then pray and open them. These [illegible] he carried out with eager care and interest, only [illegible] the terrible shock of disappointment in the end. [illegible] stages of the work of the miracle man have been [illegible] increasing doubts concerning his powers, [illegible] a nucleus of followers who hang on his lips [illegible] he utters in his disconnected sermons. The [illegible] awe which formerly characterized the [illegible] crowd toward him personally have markedly decreased, so that certain of his Italian patients seem to have threatened to "get him" for discriminating against them.

The moral which is apparent in this case scarcely needs to be pointed out. The lay public cannot easily be disturbed nowadays by the superstitions of the Middle Ages when it comes to organic diseases, such as infections and the like. General knowledge of this class of diseases has spread very rapidly, especially within recent years. The old-time medicine man has gone out of business. But when it comes to the mental side of disease there is still a lack of training upon the part of the average physician, and a susceptibility to the wildest superstitions upon the part even of intelligent laymen. In the realm of mental diseases it is [illegible] possible for dignified cults which are indefensible in the [illegible] of modern knowledge to thrive, but we are actually left [illegible] medicine men on our hands. John Cudney, [illegible] is one of them.

AN EXPERIMENTAL STUDY OF THE PERCEPTION OF OILINESS

By Lillian West Cobbey and Alice Helen Sullivan
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Introduction

In reviewing the literature on 'touch blends', we found that the only experimental work on the subject is that of M. Bentley.[1] Upon repeating Bentley's experiments, in which he paid especial attention to the perception of liquidity, we became interested in the 'oily' experience, which seemed to be a true perception, on a par with Bentley's perception of wetness. Accordingly, we determined to follow up the hint obtained in our brief experimentation by a thorough study of oiliness. First, we sought to analyze the perception into its sensory components, in order to determine the compulsory conditions for the oily perception. Secondly, we wished to synthetize our components in such a way that our *Os* should be *compelled* to perceive oiliness, even though, physically speaking, there was nothing oily present in the stimulus.

I. *Analysis of the Perception of Oiliness*

We spent four months of the academic year 1920-21 in analyzing the perception of oiliness, and the following two months in synthetizing. There were four *Os* in the first half of the work: Dr. A. H. Sullivan (Su), instructor in psychology, Miss Lillian Cobbey (Co), assistant in psychology, and the Misses E. G. MacArthur, (Ma) and S. Chapman (Ch), seniors and majors in psychology. In the second half of the work, we added a new *O*, Mr. J. P. Nafe(Na), a graduate student in the department, who served as a check on our synthetic conditions inasmuch as he had no previous experience in analyzing the oily perception.[2]

The *O*, blindfolded, sat with right arm resting comfortably on a raised arm-rest, with the middle finger of the right hand extending downward through a hole in a board which served as a finger-rest and as a means of keeping the finger perfectly still.

[1] M. Bentley, "The Synthetic Experiment," this Journal, ix., 1900, 414 ff.

[2] When the regular *E*, Co, acted as *O*, Mr. O. De Motte acted as *E*.

patient. by means of a
of the board, until the
paralyzed. The finger re-
the eyes ne *O* described his
no! no!" of course, that oily
tried again times, but they did
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paralyz experience with a
at the were more or less 'set'
almost practice in report-
encoura which was going on in
away in
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These
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The oil, kerosene oil, para-
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to be perception. We
Orleans we allowed him to shift at-
he is now naming the
and also experiences on which the
away to
treated our *Os* felt oiliness as a
keep his not at all dependent upon
instruc perception into warmth
to expe was first immersed in the oil
The and they named the exper-
charac remained in the oil, the
though warmth, and the pressure
for ev these two qualities were
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decrea for all *Os*. Examples are
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The pressure, wet under the nail."
point warmer and seems oily." (5
adays the finger; there is warmth
organ
ledge at all." (1 min.) "The finger
within there is a ring of pressure." (2
busin the tip is getting tighter." (3
is still over the finger, it is close-fitting,
and all over me like a kind of
even
not my finger, piling each roll on
light and thick (because the
with (3 min.) "The pres-
alias a suffocating warmth too,
warm even pressure, and it

Su, paraffine oil (30 sec.) "One spot on the finger feels wet, where there is a spot of cold surrounded by a feathery, lacy pressure." (1 min.) "There is pressure, very weak in intensity, which I cannot localize; there is no temperature connected with the pressure, and it no longer seems wet." (4 min.) "Snug warmth blended with pressure; the finger feels very oily." After the cup is lowered, some oil still adhering to finger: (1 min.) "The warmth is rather intense, the pressure weak, yet I do feel pressure. Still oily." (3 min.) "Hardly any sensation, just a little warmth; no perception." (4 min.) "Now there is a little cold at the tip of the finger, and a slight pressure, and it feels wet. Wetness, for me, is as if the cold crystallized and touched the finger."

Su, olive oil (30 sec.) "Cold and pressure; wet." (1 min.) "Now there is warmth and pressure and it feels oily. There is one cold spot on the back of the finger where it feels wet: but the rest of the finger is oily. The warmth and pressure seem to be woven together into a fine texture." (2 min.) "All cold has disappeared; the warmth is quite intense; as the warmth increases in intensity the oiliness becomes clearer. The warmth is deep, under the skin." (4 min.) "Still feels oily although the warmth is now less intense than the snug close-fitting pressure." After cup of oil is lowered: "It does not feel oily now; it is more like having the finger extend into warm air; the pressure is not intense enough for oil."

The significant thing about these reports is not so much the fact that all *O*s reported warmth and pressure when they experienced the oily perception, although this is important, but the fact that they all reported a *blend* of the two sensations. In looking over the reports quoted, which are very much like the reports in general, we find such expressions as these: "a warm pressure is blended well all over the finger; it is close-fitting;" "the pressure is air-tight;" "there is a suffocating warmth;" "that same warm even pressure;" "snug warmth blended with pressure;" "warmth and pressure woven together into a fine texture." These descriptions, coming as they do from different *O*s, and in spite of the fact that oils of different weights and textures were used, show that the oily experience is indeed unique, being either a pressure that is so snug or air-tight that it is warm, or a pressure that is snug because it is warm; the fusion becomes as it were one sensory quality, so that the expression "warm pressure" very aptly describes it. The snugness does not mean an intense pressure, but a light, veil-like pressure that is close-fitting.

Experiment II

When we had thrown in warm water as a check upon our *O*s' reports, in the first experiment, we found that all *O*s reported the oily perception at times when the stimulus was warm water. Our next problem was, then, to ascertain at what temperature water is perceived as oil, as well as to find, if possible, why oil and water of a certain temperature are perceived alike. Since oil was always reported as cool when the finger was first immersed we decided to use water at 32° C which should be gradually

heated, in order to simulate as nearly as possible the experience reported when oil was the stimulus.[3]

We found that all four *Os* perceived water at 32°C as 'wet'; but, as the temperature of the water increased, the wetness disappeared and the perception changed to 'oily'. Of course, there were slight individual differences in the temperature at which oiliness was perceived; but generally speaking all *Os* reported the perception when the temperature was 38° or 40° C. And always, when oiliness was reported, it was analyzed into warmth *plus* pressure. The following table gives a summary of the results for the four *Os*.

TABLE I. WATER SLOWLY HEATED ON FINGER

Obs	Temperature[4]	Observer's Report
Ch	32°-35° C	Coolness and light pressure; wet.
Ch	38°-40° C	Warmth and weak pressure; oily.
Co	32°-34° C	Cold; light pressure; wet.
Co	36°-42° C	Warmth; tight, close-fitting pressure; oily.
Ma	32°-36° C	Coolness; light pressure; wet.
Ma	39°-42° C	Warmth and light pressure; oily.
Su	32°-34° C	Clear cold, pressure in a ring and spots; wet.
Su	35°-39° C	Warmth and light pressure; oily.

We have chosen at random samples of the running reports of our various *Os* when water slowly heated was used as stimulus. The samples follow:

Ch (32° C) "There is cold and light pressure and it feels wet." (33° C) "The awful biting cold is disappearing; still wet." (34° C) "One spot on the tip of my finger is cold; the rest is warm, but the finger still feels wet." (36° C) "The finger feels cold and warm alternately; feels wet under the nail." (37° C) "Begins to feel warm all over." (38° C) "The warmth and pressure are blended now, and give a lovely oily perception."

Co (32° C) "Very cold and wet." (34° C) "The tip of the finger feels warm, with a snug, tight-fitting pressure which blends with the warmth and seems oily; but higher up on the finger, there is cold with a ring of pressure, and it is wet." (36° C) "Oily, with a warm close-fitting pressure all over the finger."

Ma (32° C) "Cold and lots of pressure; wet." (34° C) "Not so cold, and the pressure is getting light and tickly; still wet." (38° C) "Getting warm; still some pressure; feels moist rather than wet." (42° C) "Feels funny and heavy and warm and oily; it is a thin oil." (44° C) "Still feels slightly oily, but it is getting too hot and the perception isn't as plain."

Su (32° C) "There is a very intense, clear cold with little spots of pressure scattered around in it, and a ring at the top; wet." (34° C) "The wetness has disappeared except for one spot at the side of the finger nail."

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[3]We tried various devices for gradually heating the water; first we used a kerosene lamp, then an alcohol lamp, and finally we perfected an electric heater which could be placed under the cup, and which gave the water a slow and gradual warmth.

[4]We must ask the readers of this Study to regard the temperatures as approximate only. We have reason to believe that the thermometer employed had a constant error, which we have so far been unable to determine.

(35° C) "Begins to feel like oil; there is a slight warmth and pressure." (37° C) "Oily; pressure and warmth blended." (38° C) "Very good oiliness; warmth and pressure cover whole finger." (39° C) "There is definite pressure and warmth, but it does not feel oily, neither does it feel wet. I cannot name the perception, but it is rather like glycerine."

The table and the reports give added support to our belief that oiliness is a fusion of warmth and pressure. The conditions for the fusion seem to be a warmth and a pressure of such an intensity that neither one stands out from the other; it is, in other words, a matter of about equal vividness of the two qualities. In the light of our results, we may say that water of a certain temperature is not 'wet' at all but 'oily'; the pressure in the 'wet' complex is not different from that in the 'oily' complex; the latter is called snug because it is blended with warmth.[5]

II. *Synthesis of the Perception of Oiliness*

Now that we were certain of our analysis, we wished to synthetize the oily experience by stimulating a pressure spot and an adjoining warm spot with a 'dry' stimulus. We realized that our great difficulty would be to get a blend or fusion of the two sensations. We decided to work on the back of the hand where pressure and warm spots could be located easily.

Experiment III

We found pressure spots and adjoining warm spots for all five *O*s and marked these with indelible ink. We tried a small camel's hair brush as a stimulus. This we plunged into hot water, dried quickly, and then applied to the pressure spot. This method did not prove very satisfactory, for it demanded great patience and care to suit individual sensitivity. Every *O*, however, reported the oily perception a goodly number of times. Of course none of the *O*s knew whether we were using oil or something else as a stimulus; when the writers observed, we sometimes used drops of oil in order to keep them in ignorance of the actual stimulus. The following table gives the results of Experiment III.

Table II. Warm Brush Applied to a Pressure Spot

Obs.	No. of Exps.	No. of times 'oily' perceived[6]
Ch	25	10
Co	12	10
Ma	20	10
Na	30	10
Su	15	10
Total	102	50

[5]One of the writers has often observed that a lukewarm bath actually feels oily if the eyes are shut and associations are, so far as possible, banished from the mind.

[6]Since our time was limited, we set ourselves the task of obtaining, if possible, 10 cases of correct synthesis from every *O*. Hence the number in the last column is always 10.

The reports ran somewhat as follows in all the experiments:

Ch, "It doesn't feel oily until the temperature and pressure are just right; then it seems oh! so oily, just like a drop of oil on the hand."

Co, "At first there is warmth around the edge with slight pressure in the center; then suddenly the two blend and give a lovely oily perception."

Ma, "Sometimes there is just warmth and pressure, but no perception of any kind. But when the two blend into a snug warmth, or a fairly tight pressure that feels warm, it feels oily."

Na, "I get pressure and warmth and then oily—it might be warm water —no, it is more like coal oil, or even a little heavier oil than that."

Su, "At first I feel only warmth and pressure; when they 'jump together' it feels oily."

Experiment IV

Not satisfied with the results of Experiment III, where we obtained the oily perception in 50% of the cases only, we now tried a temperature cylinder for stimulus. After heating the cylinder in boiling water, and drying it, we applied it to a hair which was near a warm spot. Our pressure sensation came from the pressure of the cylinder on the hair, while our warmth came from the radiant heat of the cylinder. Here we had not only a 'dry' stimulus, but also a stimulus which did not touch the skin at all. We found slight individual variations in sensitivity, but in all cases we found that a pressure on the hair great enough to bend the hair in the windward direction until it stood perpendicular, or a little beyond the perpendicular, was necessary before the oily perception was reported. The cylinder had to be within 1/16 in. of the skin in order to give the proper intensity of warmth. When we took care to secure these necessary conditions we were very successful in our results, the *O*s failing to report the perception only when the warmth or pressure was not of the right intensity, or, in a few cases, when the two qualities did not fuse.

The following table gives the results of Experiment IV.

Table III. Hot Cylinder Applied to a Hair

Obs.	No. of Exps.	No. of times 'oily' perceived
Ch	15	10 or 66%
Co	15	10 or 66%
Ma	14	10 or 71%
Na	25	10 or 40%
Su	10	10 or 100%
Total	79	50 or 63%

Since Na entered the experiment late, and had no experience in observing before coming into the experiment, it may not be quite fair to include his results with those of the more experienced *O*s. If we omit his results from the table, we find that out of 54 experiments the four trained *O*s reported 40 cases (74%) in which they perceived oiliness.

Examples of reports in Experiment IV are:

Ch, "There is warmth and pressure; as soon as they blend it feels oily."

Co, "That is the best oily perception I have had. The oily perception comes rather suddenly, just as if a whole lot of oil were put on the hand; then later I analyze it into warmth and pressure."

Ma, "It feels like oil on a tiny spot no larger than one half a pin head."

Na, "It feels very oily on the back of the hand."

Su, "There is a smooth light pressure and a warmth which are blended. It is like a drop of oil. The drop of oil stays for some time." [The last sentence was reported after the cylinder was removed.]

It is evident from the table and the introspections that the heated temperature cylinder secures the compulsory conditions for the perception of oiliness.

The experiments above described were performed in ignorance of the work of R. S. Malmud (this JOURNAL, xxxii., 1921, 571 ff.), who attempted by simultaneous punctiform stimulation of warmth and pressure to obtain a perception of warm-wet. The attempt failed; and Malmud concludes, admittedly with some surprise, that "there seems to be no typical experience of warm-wet." She and her *Os* were, evidently, not looking for the perceptive meaning of 'oiliness'. We believe, however, that there are hints of 'oiliness' in certain of her reports, and especially in the experiments described toward the end of her paper. Malmud obtains, as we did, a true blend or fusion of warmth or pressure, and our positive result appears to be the complement of her negative conclusion as regards wetness. We hope that her experiments may be repeated with the possibility of an 'oily' quality or integration in mind.

Conclusion

1. Oiliness is a fusion of warmth and light pressure; this fusion comes as a unique experience, being, it seems, an intermediate sensory quality rather than a perception.

2. Whenever we have the right combination of warmth and pressure, we perceive it as 'oily', whether the actual physical stimulus be oil, water, or a dry, warm temperature cylinder.

3. The compulsory conditions for the perception are:

(*a*) warmth of about 38°C—40°C;[7]

(*b*) light pressure, such as that experienced when a hair is lightly stimulated;

(*c*) the two sensory qualities of such collocation and intensity that they are blended or fused into a "warm even pressure."

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[7]See Note 4 above.

[illegible]UDIES FROM THE PSYCHOLOGICAL [illegible]ORY OF CORNELL UNIVERSITY

[illegible] LOCALISATION AND THE "ATTRIBUTE OF ORDER"

By H. M. LUFKIN

[illegible] H. J. Watt proposed to add an original attribute of 'order' to [illegible] list of sensory characters.[1] We believe that the addition is [illegible]ed. A Minor Study is not the place for systematic discussion; [illegible] may touch very briefly upon the principal questions at issue.

[illegible] Watt has failed to realise that quality is itself an orderly attribute. [illegible] qualities, e. g., have their determinate 'places' in the color [illegible] and a like law holds for smells and tastes (Henning) and prob[illegible] touches (Titchener). The isolated quality (as, e. g., Watt's "qual[illegible] or mere sound") is by all analogy suspect. We expect in every [illegible] one or more qualitative series; we expect that auditory qual[illegible] show an orderly arrangement; and Watt's "order or pitch-place" [illegible] be nothing more than differentiated auditory quality. (2) [illegible] has shown, in the crucial instance of crossed and uncrossed double images, that a different spatial localisation of identical visual impressions may be explained physiologically, in terms of an 'empiristic' theory.[2] What holds of the two retinas will hold also of the two hands, etc. We need not commit ourselves outright to Kaila's view; it is enough for the present purpose to note that it offers a reasonable alternative, in the field of spatial perception, to Watt's postulate of an attributive 'order'. (3) Descriptive psychology knows less of protensive than it knows of extensive sensory experience. There is, however, so far as we are aware, no experimental evidence of an attributive basis of temporal order, while there are many experimental indications that this order is perceptive.—Aside from these special considerations (4) we cannot convince ourselves that order stands, as an attribute, on the same logical level with the acknowledged attributes of sensation.

The present Study is concerned with one of Watt's particular illustra[illegible]. "On the skin," he remarks, "it is found that every nerve-ending and every touch-spot can be distinguished from every other, with the exception, perhaps, of those that lie too close together to allow of isolated stimula[illegible]."[3] The statement repeats a generalisation of Thunberg's;[4] and Thun[illegible] is repeating, somewhat uncritically, statements of von Frey and [illegible]. These investigators, who worked only on the middle third of the [illegible] and (to a less extent) on the wrist, conclude from their results [illegible] Unterarm, *und wohl auch sonst am Körper*, wo es gelingt benach-

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[1] [illegible] The Elements of Experience and Their Integration: or [illegible] Psych., iv., 1911, 127 ff.

[2] [illegible] einer empiristischen Erklärung der Tiefenlokalisa[illegible], *Z. f. Psych.*, lxxxii., 1919, 146.

[3] The 'perhaps' seems a little mild.

[4] [illegible] *Handbuch d. Physiol. d. Menschen*, iii., 1905,

barte Endorgane des Tastsinns isolirt zu erregen, eine Unterscheidung derselben möglich ist, richtige Versuchsbedingungen vorausgesetzt" (italics ours). A little later they say: "Wir glauben..den Satz aussprechen zu dürfen, dass auf allen Tastflachen, auf welchen eine isolirte Erregung einzelner Tastpunkte gelingt, die Successivschwellen bei günstigen Auffassungsbedingungen den Abständen der Tastpunkte merklich gleichwertig sind."[5] It is clear that von Frey and Metzner are outrunning their facts by inference; but it is clear also that they are careful to distinguish inference from observed fact. Thunberg and Watt speak as if the whole bodily surface, and not the forearm only, had been explored.

We thought it worth while to test the Thunberg-Watt generalization on a part of the body that should be as free as possible from the influence of empiristic motives (visual images, reflexes). For obvious reasons, we chose the back. In a preliminary study the whole back was worked over, in the effort to find an area in which the sensory response of the pressure-spots should be attributively the same. In order that only cutaneous sensations should be aroused, we sprayed the back lightly with ether, and so adjusted the hair-aesthesiometer that, under these conditions, no sensation appeared.

We obtained the most satisfactory results from the area between the scapulae. Over a certain part of this area, moreover, lying on either side of the vertebral column between the seventh and tenth cervical vertebrae,—the part measured 33 to 40 mm. in width above, and 20 to 27 mm. below,—we found that the *Os* could not tell *whether the right or left side of the back was under stimulation*. The pressure of the hair could be roughly localized, but could not be referred to right or left of the vertebral column.[6] This result in itself led us to think that Watt's generalization would prove to be overhasty.

For purposes of tabulation, the inter-scapular area was divided into four sub-areas. The left side of the part wherein the *Os* failed to distinguish right and left we term Area I, the right side Area II. The remainder of the left Area we term Area III, and the remainder of the right area Area IV. For stimulation we had recourse to a modified Benussi kinohapt,[7] controlled by the Leipzig time-sense apparatus set to give an interval of 4/3 sec. between the stimulations; this is the interval recommended by von Frey and Metzner.[8] The duration of stimulation was 0.3 sec. The hair-constants varied for the three *Os*: B, 0.51 gr/mm; D, 0.68; H, 0.42. The current operating the kinohapt was checked several times during an observational period. At the beginning of every hour 16 pressure-spots were selected, two lying vertically and two horizontally in every one of the four areas. We never stimulated neighboring spots, but allowed at least one spot between the members of a pair. The number of spots involved in this way varied from 3 to 38; the number of intermediates was always counted. We made out 20 combinations of the 16 selected spots: vertical and horizontal within every area, and vertical and horizontal in all combinations of the areas.

The *Os* were: Dr. H. G. Bishop (B), instructor in psychology; Dr. K. M. Dallenbach (D), assistant professor of psychology; and Dr. L. B. Hoisington (H), assistant professor of psychology. D and H were highly practised in cutaneous observation; B had not observed before in a similar

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[5]M. von Frey und R. Metzner, Die Raumschwelle der Haut bei Successivreizung, *Z. f. Psych.*, xxix., 1902, 173 f.

[6]Dr. Titchener informs us that this experience of the impossibility of right-left localization at a certain place between the shoulder-blades may be had during Swedish massage.

[7]*Arch. f. d. g. Psych.*, xxix., 1913, 385 ff.

[8]*Op. cit.*, 176.

TABLE I

Percentage of Judgments of 'Same' in Series I

	Area	I	II	III	IV	I-II	I-III	I-IV	II-III	II-IV	III-IV
Horizontal	B	40	0	20	30	0	0	0	0	0	20
	D	30	30	50	70	0	20	0	0	20	0
	H	60	40	50	10	20	0	0	40	60	0
Vertical	B	10	20	30	20	0	20	0	0	0	0
	D	50	20	40	10	20	0	0	0	20	0
	H	20	30	40	20	40	20	0	0	40	0
	Average	35	23.3	38.6	26.6	13.5	10	0	—	33.3	—

TABLE III

Total Number of Stimulations and Number of Judgments of 'Same' for

Separations in mm.

Separation	0–5	6–10	11–15	16–20	21–25	26–30	31–35	36–40	41–45	46–50	51–55
B	13	28	12	10	30	15	11	5	4	5	1
	3	6	2	0	4	2	2	2	0	0	0
D	16	25	13	20	22	16	6	5	5	5	1
	8	10	4	6	5	1	0	0	0	0	0
H	20	23	15	21	17	10	18	5	4	3	3
	8	9	2	7	5	2	3	0	0	1	0
Percent. Same	38	33	20	23	20	12	14	—	0	—	0

experiment. The *Os* were instructed to attend to the cutaneous sensations set up by the stimulus, and to localize them with respect to each other. In the preliminary work the *Os* lay prone, the head resting on a cushion and the kinohapt placed over the back; but breathing led to disturbing movements which were not wholly eliminated even when the breath was held. We then tried placing cushions under chest and abdomen; but still the movements were disturbing. We then seated the *Os* in a chair, with the back supported below the area stimulated; movements were reduced, but even so were not eliminated. Finally we seated the *Os* in a chair with the back supported both above and below the area: this arrangement proved satisfactory.

After completing Series 1, in which two different spots were stimulated in every observation, we undertook a Series 2, in which (within all four areas only) a single spot was twice stimulated; the *Os*, of course, were not informed of the plan of the series. The results of both Series are given in Tables I-IV. Table I shows the percentage of judgments of 'same' in Series 1. Table II shows the same percentage in Series 2. Table III shows the total number of stimulations and the total number of judgments of 'same' for the separations grouped in 5 mm. intervals, together with the percentage of judgments of 'same' for every interval. Table IV shows the total number of stimulations and the total number of judgments of 'same', together with the percentages of judgments of 'same', in terms of the number of pressure-spots involved.

The following are some of the reports:

Series 1. *Same.* B

(1) Localized at the same place; 2 was prick, 1 was weak. (Pressure-spots, 12; separation 26 mm.)

(2) Localized at the same place; 2 was like an after-image. (Pressure-spots, 4; separation, 6 mm.)

(3) Localized in the same area; 1 was large, 2 was small. (Pressure-spots, 4; separation, 5 mm.)

(4) Localized at the same place; 2 was weaker than 1. (Pressure-spots, 4; separation, 7 mm.)

D

(1) Localized at the same place; both were weak and diffuse. (Pressure-spots, 10; separation, 20 mm.)

(2) Localized in the same area; 1 was contact, medium to weak intensity and diffuse, area was size of the thumb; 2 followed immediately, contact, medium to strong intensity, small area. Cannot say whether same or different spots. (Pressure-spots, 12; separation, 16 mm.)

(3) Localized in the same place as two contacts. (Pressure-spots, 8; separation, 13 mm.)

(4) Localized at the same place; same size and intensity. (Pressure-spots, 10; separation, 16 mm).

(5) Localized at the same place; 1 was pressure, 2 was prick. (Pressure-spots, 3; separation, 4 mm.)

TABLE II

PERCENTAGE OF JUDGMENTS OF 'SAME' IN SERIES 2

Area	I	II	III	IV
B	30	70	90	30
D	90	50	60	80
H	20	40	10	20
Average	46.6	53.3	53.3	43.3

TABLE IV

Total Number of Stimulations and Number of Judgments of 'Same' in Terms of Pressure Spots Involved

Pressure Spots	3	4	5	6	7	8	9	10	11	12
B	13 2 / 2	17 / 4	9 / 3	4 / 1	4 / 0	6 / 1	5 / 0	7 / 1	18 / 1	9 / 2
D	11 / 4	18 / 7	9 / 5	3 / 1	3 / 0	6 / 2	10 / 2	15 / 6	6 / 5	7 / 1
H	13 / 5	14 / 5	9 / 4	6 / 1	5 / 2	7 / 2	10 / 4	12 / 3	5 / 0	5 / 1
Percent. Same	29.8	32.7	44.5	23.1	—	26.3	24.0	29.4	20.7	19.0

Pressure Spots	13	14	15	16	17	18	19	20	23	26
B	5 / 0	0 / 0	3 / 0	9 / 2	8 / 1	2 / 1	1 / 0	1 / 1	3 / 1	1 / 0
D	6 / 0	4 / 0	6 / 0	7 / 0	3 / 0	6 / 1	2 / 0	0 / 0	0 / 0	1 / 0
H	6 / 0	11 / 3	4 / 2	4 / 0	7 / 1	3 / 0	5 / 2	4 / 0	3 / 1	2 / 1
Percent. Same	0	—	—	—	10.1	18.4	—	—	33.3	—

H

(1) Localized at the same place. (Most of the reports in which there was a judgment of 'same' were given in this way.)

(2) Localized in the same area; 1 was spread down, 2 was spread up. The two were not over each other, but there was no difference in the place touched. (Pressure-spots, 9; separation, 16 mm.)

Series 1. *Different.* B

(1) 2 was 1.5 cm. right of 1; 1 fell down and wabbled around, 2 was pure pressure. (Pressure-spots, 5; separation, 9 mm.)

(2) 2 was 1.5 cm. below and right of 1; same quality, intensity, and extensity. (Pressure-spots, 5; separation, 9 mm.)

D

(1) 1 was neutral pressure, followed by 2 which had the same quality as 1. 2 was first localized in the same area as 1 and then moved diagonally to the left and up ½ in. where it became itch. (Pressure-spots, 8; separation, 13 mm.)

(2) 1 was large; 2 was in the area of 1 and was small, but the centers were not the same. (Pressure-spots, 4; separation, 5 mm.)

(3) 1 was pressure, 2 was prick; the spots were different but don't know separation or direction. (Pressure-spots, 3; separation, 5 mm.)

(4) 2 was at the top edge of 1. (Pressure-spots, 10; separation, 21 mm.)

H

(1) Both touched different edges of the same spot. (Pressure-spots 12; separation, 20 mm.)

(2) 2 was just below 1. The two pressure areas touched. (Pressure-spots, 4; separation, 5 mm.)

(3) 2 was at the right edge of 1; 2 was big and diffuse. (Pressure-spots, 14; separation, 21 mm.)

(4) 2 was at the bottom edge of 1. (Pressure-spots, 10; separation 16 mm.)

(5) Localized in the same area but having different centers. (Pressure-spots, 9; separation, 16 mm.)

Series 2. *Same.* B

(1) The spots piled up on each other.

(2) Localized at the same place. Seemed to be a long *one.*

(3) Both stayed in the same place and wiggled; size and feeling of a small camel's-hair brush.

(4) Localized at the same place; was as though the hair was bent down along the back.

D

(1) Same in quality, extensity and intensity.

(2) Localized in the same place; weak, contact, diffuse.

(3) Localized in the same place; dull diffuse pressure .

H

(1) Localized at the same place.

Series 2. *Different.* B

(1) 2 was below and right of 1.

(2) Spots very close together.

D

(1) 2 was below and left of 1; 1 was neutral pressure, definite localization; 2 was contact, diffuse, not definitely localized.

(2) No definite localization; spots were perceived as different in location and quality.

H

(1) The two areas overlapped but had different centers.
(2) 2 was 1.5 cm. above 1.
(3) 2 was 1 cm. below 1. 2 was spread out and almost touched 1.

We note in Table I that stimulation within a single area gives a large percentage of judgments of 'same', and a larger percentage on the left than on the right side. Stimulation on both sides of the vertebral column (as I-IV, II-III) gives only a very small percentage of 'same'; except in the two central areas (I-II), where we had found in the preliminary work that there is no rightness or leftness, the judgments of 'same' are about half as numerous as those of the single areas. When the areas stimulated lie on the same side of the spine, there is again a large percentage of 'same', but now the right side gives the higher values.

Table II shows that, when we stimulated a single spot twice over, the *Os* reported 'same' in only about 50 per cent of the cases.

Table III shows that the percentage of judgments of 'same' decreases as the separation of the spots increases. We find, however, one judgment of 'same' for a separation of 46-50 mm.

Table IV resembles Table III. The greatest percentage is for 5 pressure-spots, but there is one judgment of 'same' for 26 spots.

Although we tried to secure an attributive identity of the cutaneous sensations, there were of course many occasions when difference was reported. Judgments of 'same' (identical locality) occurred when the sensations were attributively different, and judgments of 'different' (different locality) when the sensations, as reported, were attributively the same.[9]

Conclusions

It is clear that, whatever may be the basis of cutaneous localization (and we have no intention of attempting a theory in the present Study), Watt's generalization, offered in support of his hypothesis of an attributive order, is not valid. "On the skin it is found that every nerve-ending and every touch-spot can be distinguished from every other, with the exception, perhaps, of those that lie too close together to allow of isolated stimulation" —this statement not only, at the time of its printing, went beyond the experimental facts, but is now shown also to be at variance with experimental facts. Different laws hold for different parts of the cutaneous surface. Our results, moreover, obtained as they were on an area as free as possible from the influence of empiristic motives, suggest that localization is in general a matter rather of perception than of sensation.

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[9]The phenomenon of movement was reported on several occasions. Cf. von Frey and Metzner, *op. cit.*; V. Benussi, *op. cit.*; J. H. Burtt, *J. E. P.*, ii, 1917, 371 ff.; A. K. Whitchurch, this JOURNAL, xxxii., 1921, 472 ff.

LVI. On the Non-Visual Perception of the Length of Vertically Whipped Rods

By Erna Shults

This study supplements, on one side, the previous work of Hoisington.[1] The former study dealt with the cutaneously determined perception of the length of rods in its simplest form; this study takes up the analysis of the same perception at a more complex level. Here we inquire as to the experience had when *O* whips the rod, rather gently, in the vertical plane. Movement is usually present in our everyday experience with tools and instruments; hence we ask whether the moving stimulus more accurately conditions the perception by way of additional sensory processes or of additional sensory variables, or whether with the moving stimulus any new centrally aroused factors appear which would seem to be essential elements in the perception. The results can tell us nothing about the perception when the end of the moving stimulus-object is in contact with a surface.

The general procedure was the same as that used by Hoisington; *i. e.*, we used five variable stimuli, two above, two below and one equal to the standard stimulus; we used the two time-orders at random but in equal number; we varied the moments of length, weight and center of mass, and the *O* made comparative judgments, always judging the length of the second rod in terms of the first. We reconstructed the apparatus so that it consisted of a rigidly braced wedge-shaped basket which swung from the ceiling. Two arcs, whose radii were the distance from the bottom of the basket to the ceiling, set about 45 cm. apart in the plane of swing, made up the bottom; shallow beds on the upper side of this arc carried the rods which lay horizontally and at right angles to the plane of swing. *E*, by shifting a lever from which a cord passing over silent pulleys ran to the swinging basket, could bring any desired rod immediately in front of *O* and always at the same height; a weight acting over a pulley drew the basket in the opposite direction. This apparatus was to all intents and purposes noiseless.

The *O*s were Miss C. C. Braddock (B), scholar in psychology; S. Feldman (F), assistant in psychology; and L. B. Hoisington (H), assistant professor of psychology. B was a somewhat experienced *O*; F had had very little experience; H was well trained, having observed in the previous work. B and F were completely ignorant of the nature of the problem attacked and of changes made in the stimuli.

The instructions, which the *O*s read from a typewritten copy, were: "You will be given two rods in succession. You will take each in turn and whip it up and down twice. You are to judge the length of the second rod in terms of the first, *i. e.*, you will judge the second as longer than, equal to, or shorter than the first." After some preliminary trials the *O*s agreed that a double whip was the best; it was not long enough to give rise to much reflection or to blot out the previous experience, nor yet too short to give a clear experience. If *O* continued the whip too long, or if he felt unable to make a comparative judgment, *E* presented the pair at some later time in the series.

The serial nature of the work depended upon the different ordering of the three moments; in Series I all three moments (length, weight and center of mass) varied; in Series II length alone varied; in Series III only center of mass varied; and in Series IV only weight varied. The standard rod was 85 cm. long, the variables differed by increments and decrements of 5.5 and 11 cm.; it weighed 100 gr., the variables weighed 2 and 4 gr.

[1] L. B. Hoisington, On the Non-Visual Perception of the Length of Lifted Rods, *Amer. J. Psych.*, 1920, 31, 114-146.

more and less; it had its center of mass 15 cm. out from the front of the handle, the variables had theirs at 6 and 12 mm. farther and nearer. We took 50 comparative judgments for every variable in all series except the first, in which we took 100 judgments. After we had completed the psychophysical work we went through the series again in the same order, but this time *O* reported process in addition to giving his judgment. We hoped, by this complete separation of the quantitative and qualitative parts of the experiment, that *O* would not attend to process during the psychophysical series but would give himself up wholly to the perception. In the early part of the experiment, especially, attention to process makes the judgment less certain if not more variable. There is some evidence, however, that B sought more or less constantly throughout the whole course of the experiment for an existential basis for her judgments; it was as if there were an attempt to justify the one in terms of the other or to correlate across from judgment to process. There can be no doubt either that both B and F confused the factors of weight and length; they did not, apparently, distinguish between the greater absolute intensity of pressure due to weight and the greater relative intensity due to the rod acting as a longer lever in the hand. B, at least, finally overcame the confusion, but not till she came to the introspective part of the work; a fact which may cast some doubt upon the wisdom of the plan of separating the two phases of the work or upon the order in which they came.

Quantitative Results.—The results shown in Table I[2] are in terms of h' and L' computed according to Urban. The use of these values does not commit us on the question of the limen; it simply takes these measures as indicative of the degree to which a given set of conditions determines the perception. A small h' and a large L' stand for flat or irregular distributions, while a large h' and a small L' go with steeper curves of distribution which have no inversions, at any rate of the first order. E. G. Boring, in an unpublished communication, also suggests this use of h' in cases where the unit of measurement for purposes of correlation is in doubt. We need no more than call attention to the fact that h' and not L' is the measure which marks the steepness of the curve, and that h and L will differ from their primes according to the unit-steps, since the one is the quotient and the other the product of that unit, whatever it is. Although the lack of a known unit of stimulus with which to correlate deprives us of the true h and of the *DL*, we may, for purposes of comparison, place our five variables at equal intervals along the abscissa, and indicate them simply by the numbers from 1 to 5, when the h's and L's become in so far comparable with their own kind. Such a procedure can stand only as a makeshift in the face of ignorance; the real solution lies in complete analysis or in a new mode of attack in the case of these complexly integrated perceptions.

The psychophysical results show little that is new so far as the relative effectiveness of the three moments as conditions of the perception is concerned. The one outstanding fact brought out in this connection is the greater effectiveness of center of mass with the rods whipped up and down over that found for the bare lifting of the rods. The increments, 6 mm., are about one-half those used by Hoisington.[3] As we shall see presently, the factors of temporal variation and of spread of pressure were important on the experimental side.

The results of H show clearly the importance of center of mass as the determining condition of the perception. It is plain from H's results for Series I and III that differences of weight or of length condition somewhat, although perhaps only secondarily, the perception; it is just as apparent when we take the results for Series II and IV that it is length rather than

[2]See p. 140. [3]*Op. cit.*, 116, Table I, Series AI and BI.

weight which is the conditioning factor, a fact which comes out more clearly in a Study which is to follow. This conclusion does not hold for the other two *Os*, for whom, as we have said, weight played a large part. F, in the introspective series, almost always reported the one or the other of the rods as lighter or heavier than the other; he seemed unable to escape difference of weight even when instructed to judge length or to report process. Hence Series II yields the smallest values for *h'* and Series IV for the judgment 'longer' yields a value greater than the corresponding value for Series III, although their relative as well as their absolute magnitude reverses for the judgment 'shorter' in the same series. The results of B show little that is significant. The largest *h'* occurs for Series II, the largest average is for Series I and the smallest for Series IV, but the differences are not large when compared with the variability found between the results for the judgments 'longer' and 'shorter'. B, in all except Series IV, gives higher values for *h'* with the judgment 'shorter'; F gives the larger values with the judgment 'longer' in all except Series III; and H in all except Series I, where the difference is very slight. These results of F and H agree with those obtained by Hoisington.[4]

Qualitative Results.—A summary of the reports of B for Series I shows that the judgment 'longer' goes with more intense, bulky, massive pressures in the hand which show greater extent; the presence of pressure and strain sensations in the arm with the up-whip; a slower, more constant and greater increase and decrease of intensity of pressure with whipping; a relatively more intense pressure at the back of the hand than at the front and, at first, a considerable amount of visual imagery. The report of experience with the judgment 'shorter' gave, in general, just the opposite. The following reports are typical: (4-3—) "With the first, a bulky pressure at the base of the thumb and in the palm of the hand; the bulkiness greater and pressure more intense in palm. In whipping rod, the pressure seemed to fall back heavily on palm with a steady increase in intensity. Slight sensations of strain along back of hand and in wrist with the lift. With second rod, the same experience but less intense. The pressure was more evenly distributed between palm of hand and thumb. The rod jumped up more easily and gave a more definite pressure on the forefinger." (3-1—) "Lighter pressure in palm, on thumb and forefinger. A more surfacy, more cutaneous pressure with second than with the first. Kinaesthetic sensations in hand with both. Visual image with second of light yellowish, greyish thing which meant rod." (3-5+) "Second heavier, more bulk in hand, a bulkier pressure on thumb. Visual images of a short rod and of a longer and thicker one. Moving pressure in first seemed to come up more quickly; second seemed to be dragged down by something." (3-5+) "Pressure dull heavy on thumb, also pressure in palm and on forefinger. Sudden increase and slow decrease of pressure on finger. First, lighter; moving pressure gave less change in intensity."

F found much difficulty in reporting so complex an experience. At first, it was a matter of weight and of the angle at which the rod dropped down; later, it was a matter of comparison of fore and back pressures; the act of whipping seemed to be an inadequate stimulus for reportable experience. A few reports follow:

(3-5—) "Second heavier. Very heavy at point quite distant from handle. Could not definitely localize end point of second." (3-4+) "First heavier at end; second grew lighter toward the end." (3-5+) "Pressure more intense in second. Pressure in forefinger more intense with second than with first. Proportionately more intense in relation to pressure at base of thumb." (1-3+) "First pressure greater at base of thumb, second

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[4] *Op. cit.*, 146.

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...rn, and differences in the rela-
... With the shorter rods the
...ry less intense, and fluctuated
...r range, passing very quickly
...e total pattern while a unity
... humb and on the forefinger
... neutral quality with contact-
...ness); with the long rods, or
... ne fluctuations with whipping
... increased and decreased more
... r an appreciable time, and the
... ater part of the hand or nearly
... he following are sample reports.

... n base of thumb less intense with
... n the base of the back pressure.
... very greater in second and the
... ntense pressure with the second
... as slower with relatively slightly
... ntued pressure pattern with the
... intensity."

... e to the above; there is per-
... reports from every *O* will
... ing was a bigger experience.
... ncrease. With the first a sudden
... at base of thumb and in palm
... n intensity of pressure on the
... increased; as the rod came down
... ly" (B). (4-3—) "Pressure
... at base of thumb" (F). (3-5—)
... essure in first on forefinger greater
... (1-4+) "Fore pressure a little
... second; back pressure consider-
... he pressure pattern was more
... variation in intensity and a
... Both pressures less intense
... With first a dull pressure
... tral pressure with a hint of
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... of absolute intensity of pres-
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... d it would" (B). (3-2—)

"Lighter pressure with the second. Less subcutaneous and more cutaneous on the forefinger. Slow increase in intensity when the rod came down and quick decrease when the rod went up" (B). (5-3—) "Alternation of pressures quicker with the second than with the first. Less intense pressure with the second" (B). (2-3+) "A heavier pressure on the palm with the second. Slow alternation between the pressure on the finger and that on the thumb which gave the idea that the rod was hard to lift" (B). (3-2—) "First pressure was light and evenly distributed; second pressure at the base of the thumb was lighter" (F). (3-5—) "First pressure greater on the forefinger; second pressure lighter on the thumb" (F). (4-3—) "Pressure with the first was greater; no other difference" (F). (5-3) "With the up whip the pressure was more intense than in the second, but when the rods came down the intensity of the pressures was equal" (H). (2-3) "The immediate impression was one of slightly greater intensity on the forefinger with the second. When whipped the intensity became the same as the first" (H). (3-2) "First impression was 'shorter' but with the whip they became 'equal'. The pressure at the base of the thumb was, at first, weaker with the second but increased with whipping" (H). (3-1—) "The second experience seemed a little more diffuse but a little less extended. I did not notice any difference in intensity. It was more a judgment of inference than one of direct experience" (H).

Conclusions.—We conclude that the perception of the length of vertically whipped rods depends primarily upon the relative intensity of two opposed pressure experiences in the hand.

In addition, the most important items of experience for the perception and those which contribute most to its refinement are the differences in the frequency and rate of intensive changes with the whip.

The perception of difference in length correlates highly with difference in center of mass, and somewhat with difference in length of the stimuli.

LVII. On the Non-Visual Perception of the Length of Horizontally Whipped Rods

By A. S. Baker

In every-day life the tools and instruments which we use move or act for the most part in the vertical plane; moreover, if one gives a rod to a blindfolded subject and asks him to judge its length, he will almost invariably whip it up and down. If, then, the perception is one in which the integration of the processes depends upon past experience, we should expect the perception of the length of a horizontally whipped rod to be less refined, less accurate in terms of stimulus, than that of the vertically whipped rod. On the other hand, since the psychophysical processes have a common origin and no new or lacking moment in the stimulus can be assumed *a priori*, we might expect the perception to be as accurately determined in the one case as in the other; the usual vertical whip being a muscular rather than a perceptual habit.

We did the experimental work for this study during the Summer term of 1921. The general procedure and apparatus were the same as in the preceding study by Shults.[1]

[1]Erna Shults, On the non-visual perception of the length of vertically whipped rods, *Amer. J. Psych.*, xxxiii, 1921, 135 ff.

pressure greater at side of finger." (5-3—) "Pressure on fo than that at base of thumb in both. Difference between the second."

H reported temporal differences both in rate of ch with whipping and in the rapidity with which maxima o rod came down, followed each other; differences in re changes in intensity with whipping, with correlated differences in the extent of the pressure pattern, and d tive intensity of the fore and back pressures. With pressure at the base of the thumb was relatively less inten more rapidly and through a relatively wider range, pa from almost zero to maximal intensity; the tota patt yet showed focal points at the base of the thuml and with little pressure in between (it was often of ne fringes which gave the impression of liveliness those which gave rise to the meaning longer, the were slower in alternation, the intensity incr slowly with the maximum maintained for a pressure extended over the whole of the inne so, with a dull, draggy, 'dead' quality. The fo

(3-2—) "Pressure on forefinger and on l the second; the difference was greater in Variations in intensity with whipping relat shifts were more rapid." (1-3+) "More in especially at back of hand. Fluctuation less change in intensity. A little more ex quality a little duller at moments of great

The reports from Series III add but haps a slight difference in degree. A serve. (1-4+) "With the second the w Slight strain. Sudden increase with slo decrease of intensity" (B). (2-3+) "P bulkier with second. Also a definite forefinger. It was the way it increase it increased very quickly but decrea greater in first; in second, pressure "Pressure in first greater than in sec than pressure on forefinger in seco more intense and a little duller w ably more intense and duller. W sharply defined in extent and longer interval with whipping" (H) with the second, especially the ba fairly sharply defined in extent; contact. Less change in intensity

Series IV yields nothing sav sures. These differences proved ference of length, although th conditioned the true perception

In Series II, especi rods responded less rea tensity on the forefinger of pattern gave rise to tended to disappear a form rate of moveme "Sensations in wrist sity but it did not in

	Obs. B.				Obs. H.									
	Judgment				Judgment				Judgment					
	Longer		Shorter		Longer		Shorter		Longer		Shorter			
	h′	L′	h′	L′	h′	L′	h′	L′	h′	L′	h′	L′	[illegible]	[illegible]
Series I	.73	.62	.57	.52	1.3	.69	1.5	.37	.66	.64	.77	.33	[illegible]	[illegible]
Series II	.53	.71	.42	1.0	.42	1.9	.35	1.7	.48	.51	.46	.36	.39	.46
Series III	.44	1.3	.43	.66	.77	.51	.75	.46	.39	.69	.36	.34	.42	.46
Series IV	.18	2.2	.13	2.7	.05	15.	.06	.07	.59	.38	.71	.43	.39	[illegible]

...structor in psychology; ...ychology; H. B. Kohl- ...ollege of Arts and Sciences ...ogy. B and H were trained *O*s, ...vation; K and S were untrained. ...e: "You will be presented with two ...hem rather loosely in the hand and, ...he vertical plane, whip them back and ...You will judge the *length* of the second ...ll judge the second as longer than, equal

...s moments resulted in a four-fold division ...n which all moments varied, Series II in ...ries III in which center of mass varied, and ...ied. In the last three series the other moments ...ve stimuli. The standard rod was 85 cm. long, ...s center of mass at 15 cm. out from the front of ...nts and decrements of length were 5.5 and 11 cm., ...d of center of mass 6 and 12 mm. In any series ...moments were constant they were the same as those

...*lts*.—Table I sums up the quantitative results as ...f h' and L' computed according to Urban from results ...of Constant Stimulus Differences.[2]

...most characteristic feature of this table is its lack of ...en *O*s and more especially between the trained and un- All results agree, however, in that they gave the steepest ...rgest values for h' in Series I, also that they gave no inver- ...st order for any *O* in this series. The results of B and H agree in that they gave the smallest values for h' and both sets of ...inversions of the first order. In fact, the results of H gave a ...lue for L' for the judgment 'longer' as well as for the judgment ...he tended to judge the heavy rods as shorter than the lighter ones. ...l values of h' for the two trained *O*s show that weight conditioned ...ption very little in Series I. The fact that difference in weight ...oes not adequately condition the perception may not be accepted ...of that it might not be effective if given together with center of mass ...h length. Time did not permit the further fractionation of moments; Hoisington, in his analysis of the perception of lifted rods, found that ...ght and center of mass as co-variables gave no better ogive curves than ...l center of mass as sole variant. How far the two perceptions are anal- ...ous in this respect we cannot say. It is very doubtful whether the differ- ...nce between the values of h' for Series I and III is due in any great measure to the elimination of differences of weight in the stimuli.

The results of H and S agree in that they gave the second largest values of h' for Series III; for S they are not much in excess of the values given in Series IV, for H they are very greatly in excess of those for Series IV and considerably greater than those for Series II. Although the results of B gave larger values of h' in Series II than in Series III for the judgment 'longer', the values for Series III are considerably greater than those for Series IV; the results of K for Series III gave the smallest values of h' for that *O*.

There can be but one conclusion: the *O*s were not doing the same thing, they were not judging under the same attitude. We naturally incline to give greater weight to the results of the practised *O*s. The stimulus factors

[2]For some remarks on this use of the values h' and L' see the preceding Study.

which condition the perception of the length of horizontally whipped rods are center of mass and length; weight touches off the meaning of length, given a length and a center of mass out beyond the hand, without conditioning the real perception of 'out thereness'. This conclusion accords with the results of Hoisington[3] and Shults.

The results of all *Os* gave fair values for *h'*, considerably larger than the values obtained by Shults and very much larger than those obtained by Hoisington, when length was the only variable. This result can not be explained on the ground of different *Os*, for H served in all three experiments. His results in the study by Hoisington gave for Series A V, which was in every way comparable with our Series II, the values of .054 and .046 for *h'* and of 12.7 and 14.2 for *L'* for the judgments 'longer' and 'shorter' respectively, and for Series II in the study by Shults the values of .286 and .157 for *h'* and of 2.25 and 2.69 for *L'* for the corresponding judgments. This seems due to a fact of inertia: in the present experiment the movement was at right angles to the direction of the force of gravity and, although the weight was the same for the long and the short rods and the center of mass was the same distance out from the hand, the greater extent beyond the center of mass resulted in a different stimulus-effect as *O* suddenly forced the rod into lateral motion. This explanation finds support in the results of H, who discovered that if he began the horizontal whip very slowly he did not perceive any difference in length between two rods for which he had clearly perceived a difference when he whipped them more quickly. Other *Os*, and especially B, remarked the same fact. H, therefore, repeated Series II and began the whip of the rods very slowly. The results gave .33 and .31 as values for *h'* and 2.0 and 2.1 as values for *L'* for the 'longer' and 'shorter' judgments respectively.

Qualitative Results.—The introspective reports show that a difference in the experience of *O* resulted when he whipped a long and a short rod; that the difference was in part of the same order as that experienced when the whipped rods showed differences in center of mass; and that it resembled in part those differences experienced with the primary perception of difference in length. The greater the inertia, the greater was the temporal lag and the greater were the pressures localized in the hand; whether the pressure at the back of the hand increased proportionately more than at the fore-finger is a question to which our *Os* did not return a positive answer. According to the principles of the lever we should expect the back pressure to increase relatively more than the fore if the applied power, in the form of inertia, came out along the rod.

B reports, for Series II, as follows. (5-3—) "The striking thing was that the pressures which were heavier, more massive and more intense with the first were lighter and less intense with the second, a light agile kind of pressure." (2-5+) "Condensation or intensification of pressure came up at the end of the swing, particularly at the back. With the second, a sort of intensification of the experience in general. Strain in hand more intense; some pressure in thumb and fingers." H reports: (2-5+) "More intense pressure with whip on the fore-finger and the pressure more widely distributed with the second. Also pressure of weak intensity in wrist and slowness of change in intensity with the second as compared to the first." (5-3—) "Intensity of pressure on fore-finger less with the second. Fluctuation of intensity with whip less with second but with a more rapid rise and fall of the intensity." (3-1—) "The clear difference in the two experiences was the slightly longer duration of the maximum intensity with the whip, a little greater spread of extent and a spatial shift of pressure on the thumb with the first." (5-2—) "Less intense pressure at base of thumb and less pres-

[3]L. B. Hoisington, On the Non-Visual Perception of the Length of Lifted Rods, *Amer. J. Psych.*, 1920, 31, 114-146.

sure in the wrist with the whip in the second. Fluctuations in intensity came more rapidly and the rise and fall of intensity was faster. Less extent with the second." K, who had visual imagery as part of almost every experience, reports: (1-3+) "Not much difference. Second stretched out farther in the field. Grey did not change. Fan larger in the second case. Pressure with first a slight bit less intense than with second." (5-3—) "Very noticeable difference. Pressure more intense with first; everything seemed more intense. Fan farther away, color brightened." S reports: (1-3+) "Pressure on fore-finger and in palm of hand, kind of jerky feeling with whip. Slight pressure in wrist; more intense with second." (5-3—) "Quite a good deal of pressure in wrist and fore-finger; increased intensity with the whip. Vibrations with the second fairly short. Little pressure except with whip, then pressure on little finger and back of hand; pressure shifted with opposite swing."

If we summarize all the reports for this series we find that the absolute intensity of the pressure experience, the amount and rate of intensive change, the rapidity with which the changes take place, the presence or absence of pressure and strain sensations in the hand, wrist and arm, the extent and the shift of localization of the pressure pattern, all serve as cues to give the meaning of difference of length. All these are in addition to the primary experience which carries the meaning of 'out there', viz., the experience of two opposed pressures which stand to each other in certain ratio-limits of intensity. These are additional modalities which enter into the total complex of experience when the rod swings back and forth in the horizontal plane; they modify the meaning of length, without being able, if they stand alone, either to carry the meaning of length, or in any way to originate the perception of length.

We have given first place in our discussion to the factor of differences in the length of the rods, because this evidently is a more important factor under these conditions than it was under the conditions of Hoisington or of Shults, although Shults found it somewhat effective. Difference in weight, as found in the previous Studies, does not condition the perception; under the conditions of the present Study it may and does, with the unpractised *Os*, touch off judgments of length in the same way as has been found by the previous workers in this field. The reports show that the bare increase of intensity of pressure does not enter into the 'length complex' in the same intimate way as do the modes of experience already mentioned.

A few reports from the practised and the unpractised groups will show the difference. (1-4=) "Intensity of pressures a little greater in the second. First, neutral pressure and contact in quality; second, a little more dull, draggy. The intensity of the fore and back pressures relatively equal in the two experiences; the temporal course was the same" (H). (4-2=) "Second much less intense as a whole than the first and a little less dull in quality. Slightly less extended. Temporal course the same" (H). (4-2=) "Pressure largely cutaneous. It was measured in terms of the 'throw' of the two sticks. Rotation in palm the same, which gave the basis for the judgment" (B). (1-4+) "Great difference, like wood and steel. Pressure with first much less intense than with second. Weight of first unnoticed; weight of second seemed 5 times that of first" (K). (5-3—) "First requires more pressure to hold in hand on both palm and fingers" (S).

Center of mass, also as in the previous Studies, stands out as the one moment of the stimulus which conditions the perception more than any other. The elements of experience were the same as in the list given above: more pressure and strain in the arm, wrist and hand; more extended pressures; absolutely greater but relatively less increase in intensity; slower alternation of maximum and minimum pressures; a slower rate of increase and decrease of intensity; more intensive pressures fore and back with the back pressure relatively more intense, and very often a shift in the local-

ization of the back pressure with the longer rods and the reverse with the shorter. The shorter rods pivoted at the back, and all the movement of the rod was forward from this point; the longer ones pivoted at the fore-finger, and the movement was both forward and back of this point.

B as well as K had visual images with almost every experience. At times they approximated to synaesthesia; it was as if the pressure came in visual terms. This makes it a little difficult to exhibit parallel cases from the reports of B. He reports: "Visual sensations of pressure, a sort of synaesthesia. Shifting pressure in hand correlated with visual streaks; as the pressure jumped up in intensity the grey became black." "Pressure sensations in wrist, fore-finger and palm. All these processes fuse somehow, and coupled with the visual experience give the basis for the judgment." "Pressure and visual experience combined again. No difference in visual terms betwen hand and stick. Spread out fan-shape, extended out in terms of visual process. The pressure seemed visible." In all this, however, as in the visual imagery of K, there is no sure evidence that the visual experience is any other than processes which accrue to the pressure experience in the hand and arm, and that it is at all essential to the perception of length cutaneously given.

Conclusions.—We conclude that the perception of the length of horizontally whipped rods is even more accurate than that for vertically whipped rods.

The perception depends primarily upon the presence in experience of two opposed pressures.

The presence, in the complex of experience, of pressures and strains in the arm and hand, of the shift of the pressure pattern in the hand, of differences in temporal formation, in extent and in intensity, all contribute to the perception.

We add that it is unwise to employ untrained observers in the study of a complex experience, when the period of training is necessarily short.

REVIEWS OF BOOKS

Vorlesungen über Psychologie. By OSWALD KUELPE. Herausgegeben von KARL BUEHLER. 1920. Leipzig, S. Hirzel. Pp. viii, 304.

The sudden death of Oswald Külpe, which occurred on the 30th of December, 1915, snatched away in the midst of his productive and highly influential maturity a man whose genial personality had been impressed upon numerous American psychologists who had been privileged to meet him or perchance to study under his direction. Külpe left unsystematized and in some measure incoordinate the programme of investigation into the higher mental processes which he had been directing in his laboratories at Würzburg, Bonn, and Munich during the ten or more preceding years. It was therefore with unusual interest that one learned of the agreement that was speedily reached by certain of his former students to publish the various courses of university lectures which he had offered during the last years of his academic activity. The first of these posthumous works is now before us, and its perusal is a source of great satisfaction to those who knew the man, and who admired his critical acumen and breadth of view.

Yet there is also a sense of disappointment which can not fail to strike deeply in the minds of some, at least, who anticipated a statement, inadequate though it might be, formulating the newer ideas concerning the processes of thought which the steady stream of investigations emanating from the 'Würzburg School' had made desirable. For the lectures now published include no chapter on thought, and Bühler, who has edited the work with scrupulous fidelity to the original notes, tells us in his preface that Külpe had never lectured on this topic.

In the summer of 1909 the present writer found Külpe lecturing in his last semester at Würzburg on *Feeling and Thought*, a supplemental course offered as a complement to the general course in psychology given during the previous term. But even at this time the topic of thought was not reached, and the whole summer's course was taken up with the psychology of feeling. To feeling is devoted the last chapter of the present work, and it constitutes more than one quarter of the entire book.

Taken as a whole, the present lectures with their divisions into chapters and paragraphs follow with surprising fidelity the order, arrangement, and even the content of the lectures on psychology which the present writer heard as a student in 1902. Excepting the greater stress now laid upon functional psychology, and the implicit assumption of contents of thought, the treatment is similar, both in scope and in tone, to that of the course given thirteen years prior to the author's untimely death.

Yet so far as it goes the book is in no wise to be judged fragmentary. The plan is excellent, and the special topics are handled in a thoroughgoing manner. Data derived from a wide range of psychological investigations are marshalled together with a fine sense of objectivity, while the constructive criticism to which each topic is subjected is masterly in its evidence both of keen insight and of detailed knowledge.

At the beginning of each topic a brief paragraph summarizes the contents and conclusions of what follows. It was Külpe's practice to dictate this summary to his hearers before he entered upon a detailed exposition of his subject. There is also much of the personal touch in these lectures which, to those who knew Külpe and have sat under him, will bring back a vivid picture of the man and his method as a teacher. Even the tone of his voice seems to carry over from many of the passages.

The book is divided into five chapters. The first, being introductory, contains sections on the history of psychology, its concept and problem, followed by a discussion of psychological principles, and a study of sources and methods. We are told that psychology deals with the phenomena of experience in so far as they are subjectively determined. But although these phenomena are immediately dependent upon the ego, they are also mediately dependent upon other things, such as a nervous system. Hence, psychology must be regarded as something more than the "immediate experience" of Wundt's tautological definition, while the "intentionality" in which Brentano finds the characteristic of mentality is held to be too narrow a concept, excluding as it does the sensory, affective, and other contents of consciousness. Neither may the experience of psychology be confused with the phenomenology of Husserl; for Husserl is concerned only with explications, whereas psychology deals with factual data.

The second chapter treats of the general facts of mental life, sections being devoted to the waking and dreaming consciousness, hypnosis, the unity of mental life, the span of consciousness, individuality, psychical contents and functions. In this more than in any other chapter of the lectures Külpe touches upon the psychology of thought and the new orientation given his psychology by the investigation of this subject.

While the theory that underlies his treatment of dreams and hypnosis does not greatly vary from that of the lectures of 1902, there is an altered conception of the unity of consciousness. The significance of the *Aufgabe* having entered into the foundation of all unitary tendencies, the conception of a hierarchy of tasks, supraordinate and subordinate, is suggested and in some measure worked out. Upon closer study, the unity of consciousness becomes a multiplicity of tendencies, each unitary in its own way. Thus seven different types of unity are distinguished, the first being the unity of attention. Külpe's doctrine of attention is never clearly expressed in these lectures, and he devotes no section or paragraph to its place in his psychological system. No description is given of the content as affected by attention, though he holds that attention is limited to a few things. It can be fixated on a single content or group of contents, however, for long periods of time. The unitary influence of the attention provides a rank-order of conscious contents, a closer connection of these contents with one another, and a greater continuity of mental life. The monarchical arrangement of contents and the continuity attributable to dominating moments are resultants of attention. But what is attention itself? Presumably an act, with the diverse functions of ordering, connecting, and arranging contents both in continuous series and in monarchical groups.

A second type of conscious unity is to be found in the totalities of coexistence and sequence. Thus, we have fusion of tones, the figures and bodily forms of visual contents, the qualities of contact, and their complications. The unity of thinking that arises from the unity of a definite problem, a certain end and the direction taken by a line of reasoning, furnishes a third type. In all probability the unity of perception, remembrance, and imagination depends largely upon this characteristic of thinking. A fourth type is discerned in the unity of experience conditioned by regular and periodic happenings such as day and night, and the succession of events which repeat themselves daily. Very important is the fifth type, the unity of self: the fact that in some sense I remain the same despite the fluctuations of my experience, and that this unitary self finds its substance in the persistence of bodily sensations and the visual appearance of my person; in the psychical processes of perception, thinking, ideation, feeling, willing; in remembrances that arise from these experiences, and in the capacities which I develop through these recurrent acts and contents. A sixth type is provided by the selective characteristic of emotion: the unitary mood and temperament; and a seventh and last in the unitary direction

of volition. In the lectures of 1902 but four types of unity were distinguished; those attributable to attention, to the frequency and constancy of certain experiences, to the emotions, and to the will. The underlying theory, however, has not been greatly modified or clarified in its systematic aspect.

In considering the span of consciousness, Külpe points out the ambiguity of this problem and the complicated conditions which underly the range of awareness which chances to be under investigation. Attention is but one factor; the excitability of the organism, the nature of the ideas entertained, feelings, volition, and acts of thinking, are all important in determining the range of any moment in experience.

In the earlier lectures there follow at this point several comprehensive sections treating of the degrees of consciousness—sections which serve to introduce a detailed consideration of attention and apperception. The data of attention and the conditions both of attention and of apperception, together with a section on the theory of attention and one on practice, *Einstellung*, habituation and fatigue, are topics that do not appear in the present volume. Instead we have the scheme of the stages of consciousness which resulted from the experimental investigation of Ernst Westphal. Of interest is the comment that sensory definiteness and clearness do not necessarily change with a shift of attention. This point indicates again that attention is regarded by Külpe as an act of mind with diverse functions; it is not limited to the clarification of conscious contents, since a shift of attention does not necessarily make the content previously attended-to obscure.

As for the stages, of consciousness, we have only the report of Westphal's introspective results without a systematic treatment of their implications. First, an object is presented simply without relation to anything else in consciousness, though already it is an object. Secondly, the object is *noted*; a stage which brings no qualitative modification either in definiteness or clearness, but only a direction upon the content. The object is received: "it falls not like a stone into the water, but like a ball into the outstretched hand." At the third stage there is potential knowledge of the object. Its presence becomes an as yet unformulated knowledge about the object, and contains the possibility of naming it. The establishment of actual knowledge is, however, deferred to the fourth and last stage, in which the object is named or "nailed down." This developmental series of stages suggests, with respect to the problem of the span of consciousness, that one must distinguish the span of knowledge, the span of awareness, and the span of mere presence in consciousness; the first two being obviously of narrower range than the last. These stages also have an important bearing upon the unity of consciousness in the sense of the rank-order of contents that assemble themselves in any unitary experience.

The section on Individuality is descriptive and classificatory rather than a systematic treatment of fundamental traits resting upon an instinctive basis; but in the next section, on psychical contents and functions, we find a brief discussion of these two important groupings of psychological data. The contents are characterized as given, ready-made, as it were, to be apprehended as objective complexes, whereas the functions are the activities of the subject which perceives, remembers, cognizes, notes, thinks, loves, hates, hopes, and fears. The close connection of the two gives rise to a variety of attitudes. Although the connection is intimate we approach a dominance of function alone in states of expectancy, and of content alone in many dreams and in the absorption of concentrated effort. It is also evident upon analysis that acts and contents are independently variable, since the same content can be perceived, noted, judged, or otherwise apprehended. Contents may be directly observed, while functions can only be remembered. One can not assume an attitude of hopefulness and at the

same time observe the state of hoping, for the attitude of observation nullifies the attitude of hoping. Yet Külpe insists that functions are equally existential with the contents to which they apply: they are not to be regarded as hypothetical aids to explanation, but as immediately experienced and observed data of consciousness. The operations of the ego upon the contents of consciousness are neither hypothetical nor assumed, but are facts like any other facts. Despite this positive assertion it is difficult to understand how, through the mediation of memory, a satisfactory evidence can be gained of the psychological existence of these mental acts. Since it is admitted that they are probably at times unconscious, may they not always be so? This would in no wise destroy their actuality, though we should perhaps be obliged to admit that we can observe them only in the modifications which they produce in the contents upon which they act. Suggestive as these sections are, they leave much to be desired in the way of amplification, both in respect of pertinent investigation and of systematic theorization.

The chapter on sensation includes sections on quality, intensity, complication and fusion, contrast, and spatial character. One misses a section on time, although duration is mentioned as one of the attributes of sensation and the writer finds that the topic was treated rather fully in the lectures of 1902. The sections which dealt with the analysis of the special senses have been omitted by the editor, apparently because the notes on the sense-organs and the arousal of sensation appeared to be too meagre for publiation.

The most striking feature of this chapter is the inclusion of space among the sensations. In his description of the sense of space Külpe refers to its unique quality and its intensity, varying both in direction and in magnitude. The elementary contents of space are single extensities and distances, while the spatial complications give us surfaces, forms, and places. The impressions of space involve the special sense-organs of vision, skin, joint, tendon, and muscle, and there are spatial images of each modality. The difficulties which must be met before one can fully accept this view are then treated with remarkable candor. It is freely admitted that the intimacy of the relation of space and the sensory qualities to which it is attached make it appear more as an attribute than as an independent content. What is the adequate stimulus of space: is it objective space as such? How does it happen that different sense-organs afford the same spatial impression? These questions are not easily set aside; and it is even suggested that the spatial character of experience might be regarded as a product of the primary sensory arousal of certain sense-organs rather than as being itself an immediate sensory datum. Despite these difficulties Külpe thinks it possible to hold that space is sensory, its psychological character resting upon a phenomenological content distinct from the logical space of mathematics or common sense.

The fourth chapter deals with mental images. Though images are similar to sensations, this similarity does not necessarily apply to their order and connection. Indeed, the functional acts of the mind have a far greater influence upon the image than they have upon sensation; hence we find difficulty in placing two images in the same order of intensity or vividness. The brightest light does not blind us when it is imaged, nor is corporeality a positive characteristic of images, save possibly in certain dream-images. Yet we must sharply distinguish the meaning of the image from its phenomenological content; for the reference of a content to its object is never a psychological characteristic of the content itself. The distinction of image and sensation is further complicated by the transitional stages we meet in pseudo-sensations, in memory after-images, and in hallucinations. The distinction is therefore one which is based primarily on the non-psychological conditions of arousal. The sensations were defined as simple contents

arising from the stimulation of sense organs; the images must be different because they are otherwise aroused.

Certain fundamental conceptions are elaborated with respect to arousal. These may be enumerated as: (1) the basis of reproduction in the sensory impression, once made and retained; (2) a general tendency to give back the images of our sensations as ideas; (3) a tendency of perseveration, which favors the reproduction of some impressions more than others; (4) the association of images or bases of reproduction, which favors the reciprocal revival of the one by the other; (5) the tendencies of reproduction which are established by association; (6) the readiness for revival, which may be variously conditioned to occasion: (7) a constellation of ideas with its generative inhibitions and facilitations of imaginal contents.

The fifth and last chapter, on Feeling, is a critique of this concept, together with an evaluation of methods and results. Here again a fine discrimination of the views and the data which have been gathered by various theorists and investigators is more evident than the attempt to give feeling its proper place in a system of psychology. The two criteria which Külpe has advanced for the definition of feeling are its *universality* and its *actuality*. The first is objective in as much as the feelings have no precise anatomical basis, but attach themselves equally to sensations, ideas, and functions. The second criterion, the actuality of feeling, is indicated by the fact that the conditions and laws of reproduction do not obtain for it. Feelings are always original; they are never copies or remembrances of other feelings. While maintaining that pleasantness and unpleasantness are the two elemental qualities of feeling, Külpe proceeds to characterize different affective complexes as they are conditioned by different modes of arousal. These are the stimulus or sensory feelings, the content feelings, and the act or functional feelings. Distinction is also made between active and passive feelings, and between particular and common feelings. The active and passive feelings, differ not in quality but in respect to motor and intellectual accompaniments. Particular and common feelings are likewise of the same quality, but the former attach themselves to parts in a total consciousness, while the latter are all-pervading. Whether a common feeling is the resultant of a multiplicity of stimuli or of the spreading out of one or more particular feelings we do not know.

In summary, Külpe divides consciousness into acts and contents, the latter including sensations, images, thoughts, and feelings. It may be questioned whether he has made his case for the consciousness of mental acts; and he would have been the first to admit the need of a much more detailed analysis, both experimental and theoretical, before the functions of mind can be properly classified and their multiple influences upon the contents of consciousness adequately determined.

His treatment of the contents themselves suffers from this uncertainty regarding the attitudes under which they may be observed and defined. Recognizing as he did the tremendous influence of the functional setting upon the content observed, he seems to have preferred to elaborate upon the conditions which obtain for various types of experience, rather than to attempt a first-hand scrutiny of the phenomena themselves. Thus sensation is defined with reference to the sense-organ stimulated, and the image, though its distinction is maintained, is never clearly marked off from sensation. Feeling in turn is defined by extra-psychological means, and we can only surmise what might have been the definition of the thought content if that topic had been included.

On the whole one must conclude that these lectures were the incomplete expression of a man who was too busily engaged in the direction of a large number of research-problems to find the time in which to systematize his psychology with the precision which he himself was by nature accustomed to demand. The psychology of the higher mental processes which he

did much to establish was a field so large and so intriguing that he felt the necessity of awaiting further investigation before he could commit himself to a systematic account of it. Thus the lectures embrace much that is old, including the entire framework which he had formulated prior to the new orientation which the thought psychology gave him. But to say this is not to criticize either Külpe or his scrupulous editor. While it is doubtful if Külpe himself would have consented to the publication of his lecture-notes as we find them, we may be thankful nevertheless for their appearance, since even in their uneven state they suggest many important problems and many significant points of view. He who reads them sympathetically will discern a multitude of fine observations that will contribute substantially to any serious attempt to construct a psychology adequate to the demands both of our existing knowledge of the subject and of our ever-widening field of research.

Cornell University R. M. OGDEN

Suggestion and Autosuggestion. By CHARLES BAUDOUIN. Translated by E. and C. Paul. New York, Dodd, Mead and Company, 1921. Pp. 349.

Baudouin is a pupil of Coué, who has been carrying on a large clinic at Nancy since 1910. The book is a theoretical exposition of the basis of Coué's practice, which is autosuggestion. The chief obstacle to autosuggestion lies in the fact that, under ordinary conditions, the more we try to concentrate our attention on the idea we need, the more attention tends to waver between this idea and opposing ideas. The man who was told that he would find buried treasure if he could dig without once thinking of a certain tabooed phrase had little chance of success. Thus voluntary autosuggestion reverses itself, according to the law which Baudouin has styled that of reversed effort. Autosuggestion must not begin with an effort of 'will', but by a method of relaxation which brings the 'subconscious' into play. One puts oneself into a restful attitude and tries to think of nothing at all. After a time one repeats to oneself the suggestion one wishes to accept. The preliminary relaxation is the essential thing in autosuggestion as in heterosuggestion. The translators, by the way, have translated the difficult word '*recueillement*' rather unhappily as 'collection'. The ordinary translation 'concentration' would have been quite as good, but 'withdrawal' would perhaps be better, since '*se recueillir*' means not a direction outward of the concentrating powers of attention but a gathering of them inward.—What is the advantage of autosuggestion over heterosuggestion? The two evidently are essentially one in nature: every accepted suggestion from without becomes an autosuggestion. But autosuggestion is free from the hampering suggestion that someone else is necessary to the situation, a freedom that certainly is most desirable.

Children, however, are not allowed the precious privilege of autosuggestion. A chapter on their education presents us with the familiar picture (not often, let us hope, realized in life) of the mother bending over the sleeping child and murmuring suggestions into its ears. We teachers, who give suggestions to the young, pacify our consciences by the comforting thought that they will reject what does not naturally belong to them. We can but be thankful that we, ourselves, were allowed to grow up without ever having lost our normal powers of resistance to our parents.

Vassar College MARGARET FLOY WASHBURN

Elements of Folk Psychology: Outlines of a Psychological History of the Development of Mankind. By WILHELM WUNDT. Authorized translation by E. L. SCHAUB. London, George Allen & Unwin Ltd.; New York, Macmillan Co. 1921. Pp. xxiii., 532.

It is encouraging, as a sign of the times, that this translation has had a second printing. The exposition of the *Elemente* is simple;

the book covers an enormous range, and is packed full of ideas; and the general trend of its doctrine is sound. The writing is also singularly fresh; only in the constant recourse to dual division do we note any hint of stereotyping in Wundt's thought. Occasionally, of course, we have a one or a three; but the twos—two reasons, two motives, two factors, two forms, two conditions, two changes, two ideas, two lines of development, two conceptions—confront us on every other page. This tendency is, undoubtedly, much more than a mannerism: one is reminded of the illuminating articles by Karl Groos *über den Aufbau der Systeme* (*Zeits.*, 49 and later), —articles which, to my knowledge, have not yet received the attention they deserve. For the rest, Wundt's style is mature, weighty, assured; and his sentences are interlocked in the characteristic way that makes hard the path of the translator.

The new issue is offered to the public as a 'revised edition', though we are not told where the revision has wrought change or how far it has gone. I do not think that it can have been very thorough. At all events, every single one of the passages that I had marked for correction or query in 1916 remains unchanged in 1921. Even the misprints are left standing. I subjoin (with page-references) a partial list of these passages.

32 For *imititate* read imitate.
38 For *Lewes Morgan, Ancient Humanity*, 1870 read Lewis Morgan, Ancient Society (cf. 152), 1877.
42 (line 5 from bottom) for *polygamy* read polygyny.
75 The Sarasins published their book on the Veddahs in 1891, so that Wundt in 1912 should have dated its appearance "about twenty (not ten) years ago." In any case the *ten* should have been corrected in 1921. So the "sixty to seventy years old" of p. 109 should have been changed, or its date (1912) added. The phrase "some two years ago" on p. 258 does not occur in Wundt's text, 1913, 256.
79 For *Sea of Bengal* read Bay of Bengal.
96 The ring-tailed lemur belongs to Madagascar; the Malayan *kra* is the Macacus cynomolgus.
117 (9 lines from bottom) For *particular* read individual; cf. the use of particular for *bestimmt* on the following page.
124 For *cooking* read boiling.
177 *Boomerang* is either a slip on Wundt's part for bull-roarer, or indicates a faulty memory of Spencer and Gillen. Churingas of boomerang-shape are altogether exceptional.
180 *Aranda* is the German of the English Arunta.
197 For *casual* read causal.
198 *Indo-Germanic*, here and elsewhere, should be Indo-European.
212 For *prepondering* read preponderating.
218 For *Mycenian*, here and elsewhere, read Mycenaean; and for *Mycene* (p. 377) read Mycenae.
223 (line 16 from bottom) For *he* read it.
237 The Polynesian child has to live "several hours" to gain its right to existence; on p. 44 the child has to live "but a single hour."
242 For *Preuss*, here and elsewhere, read Preuss.
249 For *Zuni*, here and elsewhere, read Zuñi.
255 For *Eleusynian* read Eleusinian.
297 For *Opis* read Apis.
325 For *testify* read testifies.
331 For *older members* read other members.
355 Is it worth while to change our familiar phrase "by the oaks of Mamre" to "near the terebinths?"
363 (line 10 from bottom) The punctuation is doubled.
417 For "peoples of the Andes" I should prefer "Andean peoples," since the associations of the word Andes are to South America.

422 (line 17 from bottom) For *it* read is.
449 (line 12 from bottom) For *heterogeny* read heterogony.
465 For *phallephoric* read phallophoric.

Finally, to conclude this list of little things, I note that 'will' and 'would' continually take the place of 'shall' and 'should:' cf. 68, 96, 114, 151, 192, 227, 241, 258, 267, 331, etc.

There are, however, larger things also to be considered; and of these there are two (as Wundt might say) that have specially impressed me. I suggest, first, that it would be well to give the full titles, dates, places of publication and (if these exist) titles and dates of translations, of the books which Wundt mentions casually and characterizes incompletely in the course of his exposition. Wundt's references, which naturally tend to be German, might also be supplemented by a few of our first-rate English books. A selected bibliography would surely be of great aid to the serious student; and if the publishers should object to an extension of the volume, there is a blank page 524 asking to be filled. I suggest, secondly, that in the cases where Wundt trips over a matter of fact—the generalization concerning Australian shields on p. 125 (cf. 299) is a flagrant instance—the translator should try to discover the source of the mistake, and in a footnote should state it and correct it by giving chapter and verse of some more reliable authority. It is impossible that a book of this sort should be errorless, but the errors lie on the surface.

E. B. T.

The Biological Foundations of Belief. By WESLEY RAYMOND WELLS. Boston, Richard C. Badger, 1921. pp. xi., 124.

The author has here brought together five essays, previously published in periodicals, which have as a common theme some aspect of religious belief. The first essay, which points out the "biological utility of religious belief during the course of human experience," gives instances and deductions from history which, if accepted, prove that the human races which have survived owe their survival as well as the institutions of art, industry, science, law and politics to the fact that they possessed some form of religious belief, quite independently of the question whether those beliefs were true or false. The argument leaves us a little uncertain whether belief is the foundation of biology or biology the foundation of belief. Religious belief, we infer, stands as the 'cause' of biological survival, although in a later essay the survival of belief points to its biological basis.

The second essay, which treats of two fallacies, the *pragmatic fallacy* and the *fallacy of false attribution*, is of a higher order and of more concern for the philosophy and psychology of religion. The first of these fallacies is the fallacy of assuming to be true that which carries value or that which works; it is due to the failure to recognize that beliefs which are false may at the same time be of great value. A 'metaphysical' belief, whether true or false, has the same subjective effect or value; if it be a 'scientific' belief and untrue, the objective results will, if one persists in the belief and acts thereon, more than outweigh the subjective, and hence leave a balance on the side of disvalue. No attempt is made to apply in detail this mode of characterizing science.

If the discussion of the *pragmatic fallacy* leads on to questions of the relation of mind and body and perhaps inevitably to an organismic or behavioristic point of view, the *fallacy of false attribution* points to the problem of meaning on the one hand and of adequate stimuli on the other. The meaning which attaches to an experience is neither the experience itself nor the adequate stimulus for the experience, and the attribution of the experience to a supernatural source simply because the experience carries a meaning which points to the supernatural, even in the face of an adequate

physiological explanation, is the common error of mysticism. The mystic steadfastly refuses to define God as the mystical experience; He is "not the experience but the giver of the experience." As the author rightly insists, the experience as experience is a fact wholly independent of what the experience means. The full comprehension and avoidance of these two errors should clear up much confusion and obviate common errors in metaphysical thought; not, again, that 'metaphysical' beliefs must be true to have value or to survive; on the contrary, their value and survival power are independent of their truth or falsity.

We may, as we saw above, differentiate 'scientific' and 'metaphysical' beliefs since the one does and the other does not have objective effects. In a similar manner we may differentiate religious values from those of the other value-disciplines on the basis of "the objects to which they are said to attach." Having thus isolated religious values the author offers a classification of them "which will make clearer what would be an objective, behavioristic account of religious values. "It is a classification logically developed and concretely applied.

But beliefs are modes of response or organic attitudes, either positive or negative and, although truth does not, properly speaking, attach to beliefs, but only to propositions, we may call a belief true or false when the response accords with the truth or falsity of the proposition responded to. Add to this the further statement that the instinctive-emotional nature of man determines the response, and you have one of the primary bases for the biological foundation of belief. The instincts, however, singly or in integrated complexes, pure or defiled by accumulations from experience (if they are anything other than the objective responses) are not religion: religion requires in addition "a belief as to the reality of some more or less supernatural object or objects about which these instincts are united into a religious complex." We thought that "belief consists either of an actual response or of an organic attitude." Here is surely definition from two different points of view. The author proceeds: "But, without such instincts *as driving forces* (italics ours) in human life, religious belief would not exist among men." What, then, is an instinct? and what is a belief? We are not greatly helped by the statement that "so far as questions of positivity and negativity in the behavioristic sense, and truth and falsity in the logical sense are concerned, belief and judgment are practically interchangeable."

The second argument in favor of a biological foundation of belief is the survival value, the almost universality, the permanency through change, the self-evident nature of certain beliefs. This could be, so the argument goes, only on the hypothesis of a biological foundation. The adoption of the behavioristic point of view makes such an evolutionary basis defensible.

The concluding essay presents in general outline the principles which should obtain in religious and moral education. The system rests almost entirely on the doctrines of recapitulation and of sublimation, although the latter doctrine supposes the former as a basis.

L. B. Hoisington

Psychology for Normal Schools. By L. A. Averill. New York, Houghton Mifflin Company, 1921. Pp. xx, 362. $2.25.

In this book the prospective teacher is told what instinctive behavior is natural to the child, and what emotions are when emotion is regarded as a phase of instinctive behavior. Sensation, perception, attention, memory, imagination, thought are all presented as activities or as reactions to external stimuli. The reader is encouraged to look for manifestations of these activities in the child, and instances of their normal occurrence are cited. So

far the author is consistent with the general purpose of the book, to present a psychology of childhood, but a discussion of heredity, which he now introduces, seems to be beside the point, because it does not add to the store of knowledge about the behavior of children to know that they have probably not inherited their parents' acquired characters. If we read beneath the surface, however, we find that the unexpressed mission of the book is to plead for the educational and moral welfare of children, which welfare is to be obtained by means of a thorough and sympathetic knowledge of children and by control of their heredity. Besides the straightforward account of behavior to which we have referred, other material is added to extend the prospective teacher's knowledge of the nature of the child. There are chapters dealing with the exceptionally bright and the exceptionally dull child; with psycho-neuroses and the unstable nervous system; with the delinquent child; and with the period of adolescence. These are followed by the final chapter on the "evolution of the social attitude toward children." It is this chapter which indicates most conclusively that the author desires to do more than write a psychological account of childhood.

Such a book is not strictly a psychology of childhood; but it is a faithful account in very elementary terms of the topics with which it deals. It is so elementary that it tells but little of behavior that any observing eldest child in a family has not noted for himself. Since, however, it is intended for pupils in normal schools who have just completed high-school, perhaps its simplicity is not too extreme.

H. G. Bishop

NOTES

BENNO ERDMANN

Professor of Philosophy at the University of Berlin, and member of the Berlin Academy of Sciences, Geh. Reg.-Rat. Benno Erdmann died from an embolism on January 7, 1921, in his seventieth year. For two years his weakened heart had been a source of inconvenience, making the daily trips between the University of Berlin and his home in Gross-lichterfelde an increased burden. Yet he remained in active service almost to the last, working with his accustomed zeal to within three days of his death.

Many Americans have felt the spell of his incomparable lectures. Not a few were trained simultaneously in the philosophical disciplines, in the methods of productive scholarship, and in the art of university instruction in his seminars. These latter have all felt something of the charm of his wise and infinitely patient friendship. Those who knew him in his lectures and his seminars will think of him first, I believe, as a great teacher. He lectured without notes. Yet the perfect form of his utterance made it comparatively easy for those who were still struggling with the German language to follow and understand. The logical sequence of his thought gave his lectures a naturalness and inevitableness that I have never heard equaled. They seemed unforgettable. The complete mastery of his material, his candor, enthusiasm, and magnetism combined to fill the larger lecture rooms, whether he lectured on Psychology, Logic, the History of Philosophy, or Education.

It was, however, in his seminars that we got our real insight into the extent and accuracy of his scholarship, our inspiration to productive thinking, and our training. The limitations of the lecture irked him. It offered no opportunity for real contact with the minds to which he spoke. He seemed to envy the relative informality of American class-rooms. It was in the freedom of the seminar that he brought out the best that was in each one of us to meet the great problems of life and thought. How often have I gone back to my room full to bursting with enthusiasm for solving the apparently all-important problems that we had just gathered the relevant data for understanding!

But of all those who called him teacher and friend I believe that I have most to be grateful for. It was a seminar problem in the technique of investigating the psychology of reading that started it. For more than two years we worked together almost every week-day for one or two hours in the diminutive Psychological Institute or at his home. It was in those hours of tireless work that I learned the sacredness of an experimental fact, the high obligations of a scientist, and the demands of scientific evidence. Few modern students have had the privilege of observing so intimately the operations of the mind of a master attacking a scientific problem.

Though he visited America once, to participate in the scientific discussions of the St. Louis Exposition, most American students knew Professor Erdmann only through his writings. They knew him consequently only as an accurate and profound scholar. It seems to me that scientific style never more completely hid the charm of personality than his. His first well-known, and probably still his best-known works in America were his studies of Kant's *Kritizismus*, and his edition of Kant's *Reflectionen* and *Kritik der reinen Vernunft*. It was these that took me to Halle. In

recent years he made frequent reference to his *Logik*, the *Psychologische Untersuchungen über das Lesen*, and the reformulation of J. E. Erdmann's *Grundriss der Philosophie*. The latter was regarded by him as a work of piety, an interruption of his main tasks, though he was peculiarly fitted by temperament and training for its successful execution.

To his mission [illegible] he followed through and embodied in his published works, his respect [illegible] as the universal scientific conditions of reality. Before his death he had completed most of the separate investigations that seemed to belong to the whole. Only the second part of his *Logik* is unfortunately lacking. This probably exists in manuscript and, as many of us hope, may yet appear.

As the writer of his scientific [illegible] about a psychologically conditioned [illegible]. His historical studies of the conditions and inner development of the Critical Philosophy of Kant, his *Logik*, the St. Louis lecture on the [illegible], and the numerous studies of the thought-processes and their correlation with language, including our study of reading, appear as necessary steps in the intensive study of the various phases of a central problem, whose presentation in a single unit was never published if it was ever written. I think he feared the psychological dangers of system-building. The nearest approach to a unitary presentation appears in his *Wissenschaftliche Hypothesen über Leib und Seele*. It may be classified as a thoroughgoing phenomenalistic parallelism.

Aside from the works already mentioned, one should include in the list of constructive special studies his too little known *Zur Theorie der Apperception*; the *Zur Theorie der Beobachtung*; *Die psychologischen Grundlagen der Beziehungen zwischen Sprechen und Denken*, all three published in the *Archiv für systematische Philosophie*; the *Umrisse zur Psychologie des Denkens* in Sigwart's *Festschrift*; and the recent *Grundzüge der Reproduktionspsychologie*.

A bibliography of his numerous papers, *Festreden*, and editorial work lies beyond the scope of the present notice. But it would be materially incomplete if it failed to give a glimpse of the wonderfully kind, great-hearted, unaffected personality that bound his pupils with ties of deepest affection. Though his guest both in Bonn and in Berlin, I knew him most intimately in Halle. There he permitted me to accompany him on some of his errands of mercy that I suppose were utterly unsuspected by any of his colleagues, unless possibly by the inseparable Breslauer group, Pischel and Eduard Meyer. His personal interest in the members of his seminars and their affairs was evidenced by innumerable incidents. The American stranger found a real home open to him, and a real home-welcome, in which the entire family participated. Three Christmas Eves I was invited to the sacred family festival, not as an onlooker but as participant, with my own little table of gifts, *Wurst*, and Christmas cake. I shall never forget the beauty of his face as the pungent odour of spruce needles, fired by the artful placing of the candles, revived memories of his childhood as only odours can. A revelation of his interest in my physical welfare came one unforgettable Easter when he decided that his laboratory assistant was over-fatigued, and led him into the Harz mountains, quietly regulating the stopping-places by his knowledge of my meagre resources. It was a strange coincidence that I broke the long and mutually painful silence which was necessitated by the war on January 7th, the day he died. Perhaps the finest indication of his personality and of his practical philosophy was the tender and profoundly significant question that he asked his wife only a few hours before his death: "K[illegible]est thou how to distinguish between the physiological and [illegible]gical?"

Raymond Dodge

[illegible]ersity

FESTSCHRIFT FOR CARL STUMPF

Professor Stumpf's pupils presented him, on his seventieth birthday, April 21, 1918, with a manuscript *Festschrift*. Since it was found impossible to publish the work as a volume, the articles composing it are distributed in the technical journals. We have noted the following contributions.

(1) K. Koffka, Zur Theorie einfachster gesehener Bewegungen: ein physiologisch-mathematischer Versuch (*Zeits. f. Psych.*, lxxxii., 1919, 257 ff.). Attempts, on the basis of Wertheimer's physiological theory of seen movement, to translate Korte's laws into mathematical terms.

(2) A. Gelb, Ueber den Wegfall der Wahrnehmung von 'Oberflächenfarben' (*ibid.*, lxxxiv., 1920, 193 ff.; enlarged in publication). Describes cases of brain-injury which have led to the loss of surface colors. Since the object-consciousness is intact, the remaining film-colors maintain their color-constancy as *Sehdinge*.

(3) H. Friedländer, Ueber Gewichtstäuschungen (*ibid.*, 258 ff.). Illusions due to material are based on the idea of expectation, illusions due to volume on this and on the influence of density. The 'idea of expectation' involves a complex play of the associative mechanism, and may work by way of positive suggestion as well as by contrast.

(4) N. Ach, Zur Psychologie der Amputierten: ein Beitrag zur praktischen Psychologie (*Arch. f. d. ges. Psych.*, xl., 1920, 89 ff.). Discusses the mental state and the psychological treatment of the patient; the choice and use of the prosthesis; and the education of the will to work.

(5) O. Lipmann, Die psychische Eignung der Funkentelegraphisten: Programm einer analytischen Prüfungsmethode und Bericht über eine Experimentaluntersuchung (*Zeits. f. angew. Psych.*, xv., 1919, 301 ff.). Analyses the work of the operator into its psychical part-functions; receiving is a more complicated matter than sending. Describes a test-apparatus, whereby the work of the receiver is reproduced in simpler form and under measurable conditions. Outlines two methods of evaluating the results.

(6) O. Abraham, Zur Akustik des Knalles (*Annalen der Physik*, 4te Folge, lx., 1919, 55 ff.). Explains the noise (pop) accompanying very brief siren-tones or noises as due to the formation of a single larger wave which includes the whole number of vibrations in a new physical unit.—

Professor N. Ach of Koenigsberg has been good enough to complete the list, as follows:

(7) A. Guttman, Beobachtungen und Erfahrungen über Intonation (*Beiträge zur Anatomie, Physiologie, Pathologie, Therapie des Ohres, der Nase und des Halses*, herausg. von Passow und Schäfer, xv., 1920, 81ff.).

(8) E. M. von Hornborstel, Ch' ao-t'ien-tze: eine chinesische Notation und ihre Ausführungen (*Arch. f. Musikwissensch.*, i., 1919, 477 ff.).

(9) G. Schünemann, Kasan-tatarische Lieder (*ibid.*, 499 ff.).

(10) M. Wertheimer, Ueber Schlussprozesse im produktiven Denken, Berlin and Leipzig, 1920.

The remaining contributions to the *Festschrift* were unpublished at the time of Professor Ach's communication:

(11) K. Lewin, Psychologische und sinnespsychologische Begriffsbildung.

(12) D. Passau, Die Gefühlslehre bei Joh. Nik. Tetens.

(13) O. Pfungst, Zur Psychologie der Sanitätshunde.

(14) J. B. Rieffert, Ueber das Verhältnis der funktionspsychologischen zur reproduktionspsychologischen Psychologie.

(15) H. Rupp, Die Gedächtnisfarben und analoge Erscheinungen verschiedener Sinne.

E. B. T.

THE EDINBURGH MEETING OF THE BRITISH ASSOCIATION

The British Association for the Advancement of Science held its eighty-ninth annual meeting at Edinburgh from September the 7th to September the 14th, 1921. For the first time Psychology had an independent section. The President of this section, Professor C. Lloyd Morgan, acted as Chairman throughout the meeting. At all times there was a large and interested audience. In fact, I understand that the Psychological Section was one of the most largely attended. Many more non-scientific members attend the British Association than is the case with the American Association. The former Association offers numerous opportunities for the members to see the surrounding country and points of interest. Many laymen, therefore, come from a long distance and intersperse their holiday activities with educational pursuits. The twenty or thirty scientific psychologists present at the meeting seemed like a very small group in comparison with the semi-popular audience. The speakers undoubtedly suited the style of their papers somewhat to the interests of their hearers. For the most part the papers were very general. Only two of them could be classed as reports of experimental work, and one of these was in the applied field. There were in all sixteen papers, beside two symposiums and the presidential address, of which eight were upon applied subjects and eight upon fundamental problems. That is, half of the papers were in the applied field. Even these figures, however, do not give an adequate idea of the great interest in applied psychology in England at the present time. This interest was shown still more clearly by the response of the audience during the discussions. The wave of application seems at present so engulfing and so many of the good men are being drawn into that field that an experimentalist at the meetings could not help feeling some apprehension.

Beside this tendency toward applied psychology and the very general nature of the papers three points impressed me: the influence that Freud has had upon British psychology, the style of the papers, and the nature of the discussions. The frequency with which reference is made to Freud is indicated in the following description of the papers. The distinguishing feature in the style was the fact that, in many instances, the articles were written in essay-form with attempts to please the audience by well-turned phrases and appropriate epigrams. One might wish for more substance, but perhaps it is impossible to hope for more details at such a semi-scientific assembly. There were only four papers at one session, and this arrangement left sufficient time for discussion, which was encouraged by Dr. Morgan who spoke a few words of friendly praise interspersed with criticism after each paper. At times the discussion became very sharp and remarks were made which would have aroused lasting enmity in America. But the British do not seem to consider remarks made upon the platform to be personal. It appears that the spirit of political campaigns has been carried over to the scientific meetings.

The presidential address was upon "Consciousness and the Unconscious." Dr. Morgan explained his 'emergence' theory of consciousness and dealt with the criteria of consciousness on the lines of what he conceived to be its evolutionary genesis. Essential to consciousness is a memory of the past or "againness" and expectancy or "comingness". I understand Dr. Morgan is writing a book on the subject which is to be finished by spring.

"Vocational Training" was discussed at a joint meeting of the psychological, educational and economic sections. Dr. C. W. Kimmins, Chief Inspector of the Educational Department of the London County Council, advocated the use of intelligence-tests in the selection of defective children for special schools and in the awarding of scolarships. He also referred to the value of vocational tests in schools. Mr. D. Kennedy Fraser, Lecturer

in Education, Edinburgh University, thought that vocational education was needed in order to train the large number of children who were misfits in a uniformly difficult curriculum. Dr. C. S. Myers, Lecturer in Experimental Psychology, Cambridge, suggested that cinematograph films be used in the schools in order to acquaint the children with the various forms of occupation, and that such exhibitions be accompanied by lectures upon the duties, responsibilities, dangers and prospects of the various occupations. Mr. F. Watts, Stockport, criticized the use of vocational tests in the present state of society. He considered that industry should be brought into vital contact with the schools. Sir William Beveridge, Director, London School of Economics, said there were three distinct economic consequences in connection with vocational selection: unemployment would be diminished, people would stick to their jobs longer, and production would be increased. Miss L. Grier, Cambridge, referred to problems concerning the women in industry, and advised giving vocational training in schools, factories or special institutions. Judging from this discussion, from the criticisms which followed from the floor, and from other papers at the meeting, it would seem that, although the British psychologists are acquainted with the literature of the subject, applied psychology is at about the stage of development that it reached in America five years ago.

The second symposium, which took place at a combined meeting of the Zoology and Psychology sections, was upon "Instinctive Behavior." Dr. J. Drever said that behavior, even in the lower animals, involves a psychical factor which can not be explained in physical terms and which has to be considered in an adequate scientific account of instincts. Professor E. S. Goodrich, although admitting that there is no fast line between instinctive and intelligent behavior, accepted plasticity as a criterion of intelligence. Professor Arthur Thompson presented a diagram of the different grades of instinctive behavior. He described the difference between instinctive behavior that occurs in one continued series of relexes and that form which consists of an interrupted chain of activities. In instinctive behavior, which is persisted in despite interruptions and difficulties, one should look for psychical factors. Professor Lloyd Morgan, in opening the discussion which followed the papers, said that, according to his belief, there is no intelligent, rational behavior that has not its instinctive basis. Dr. Chalmers Mitchell made a violent attack upon all the speakers for accepting a factor like consciousness or intelligence before they had exhausted the other factors. As will be seen from the above, old ground was plowed over, but not very fruitful soil was uncovered.

Professor T. H. Pear, Manchester University, in his paper upon "A Neglected Aspect of Forgetting," gave a classification of the different types of forgotten memories. He emphasized particularly those memories which are forcibly barred from the personality. He said that inasmuch as there seems to be a close relation between sentiment and complexes, repression is an important factor in sentiments.

Dr. J. Drever, in his paper upon "Appetition and Reaction," introduced Freud's distinction between the pleasure-principle and reality-principle. We have appetition when agreeable experiences are sought and disagreeable experiences are shunned with reference to no end beyond the affective. We have reaction when the action is determined with reference to an external object or end, independent of any affective factor. This distinction is similar to that of Freud. On the other hand Freud apparently considers appetition more primitive than reaction. Dr. Drever believes the safest view for the psychologist to take is that probably both are equally primitive.

Dr. W. Brown, Cambridge, presented a long paper upon "Psycho-analysis and Suggestion." The starting point of his discussion was Freud's assertion that suggestion is a form of transference. The paper for the most

[illegible] interruptions of shell-shock cases and other forms of [illegible], who seemed to be in close touch with Amer- [illegible] "Some Suggestions as to a Common Ground [illegible] Psychology." Miss E. L. G. Ross's [illegible] Vocational Fitness Among Mental [illegible] cases were taken as subjects, and [illegible] compared with their industrial ability and [illegible] factors were studied on the basis of [illegible]

[illegible] "A Plea for the Psychological Treatment [illegible], in discussing the "Present Con- [illegible] that it is not enough to analyze and [illegible] Industrial conditions will [illegible] instincts of man. The writer [illegible] has been done in industrial psy- [illegible]

[illegible] Feeling." He distinguished [illegible] relaxation in response to a [illegible] a response to one unfavor- [illegible] discord induced by [illegible] higher and lower [illegible] sentiments from different [illegible] are accompanied by their [illegible]

[illegible] "The Study of Personality." [illegible] laboratory work, and [illegible] from the var- [illegible] obtained in an [illegible]

[illegible] extensive research [illegible] of a number of [illegible] a surprising number of [illegible] in certain [illegible]

[illegible] account of "Social [illegible] a diagram showing [illegible] medical and crim- [illegible]

[illegible] an experiment upon [illegible] scored much [illegible] wheelbarrow. [illegible] would be ex- [illegible]

[illegible] of the Sense [illegible] changes in the [illegible]

[illegible] Mr. [illegible] Fa- [illegible] of [illegible]

[illegible] September, [illegible] a joint meeting [illegible]

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AN EXPERIMENTAL STUDY OF CHILDREN AS OBSERVERS[1]

By Claire Comstock and Helen Kittredge

The current text-books of child psychology seem to be made up of (1) a summary of observations, often of the most casual sort, taken by differently trained observers on different individual children (*i. e.*, varying as to age, sex, heredity, environment, etc.) and under quite different conditions, (2) a presentation of statistical results of studies made of groups of children in classrooms, including mental tests; and, finally, (3) that admixture of sentimentality without which no text-book of child-psychology appears to be complete, but which would be ridiculed in a treatise of general psychology.

Much of the work has been done with single children, since the emphasis has been largely on the development of the child-mind, and it has accordingly been thought better to observe one child carefully from birth to early adolescence than to work with numbers of children. These studies of individual children[2] are interesting and valuable to child-psychology. They seem, however, to be but the beginnings of a child-psychology. Different investigators reach different conclusions and, at best, the number of reliable studies of this sort is small. Group-studies have concerned themselves, for the most part, with investigations of the "learning process" and mental tests, and have had pedagogical applications as their goal. Few of the studies are comparable with the experimental studies of the "adult mind," and therefore the text-book of child-psychology

[1]From the Psychological Laboratory of Smith College.
[2]Cf. W. Preyer, *The Mind of the Child*, 1890; M. Shinn, *The Biography of a Baby*, 1900.

has not as a basis the results of experimental work which make possible the definite statements of fact in general psychology.

One reason for this difference is the tacit assumption that the child cannot describe his experiences, and that he cannot and should not be brought into a laboratory where the conditions of experiment can be controlled and repeated under exactly similar conditions with other children. Many writers declare that the atmosphere of a laboratory is unnatural and that the child brought there becomes, for the time, an unnatural being. The first objection—that children can not describe their experiences—seems more serious. But Binet in 1903 writes of his work with Armande and Marguerite: "Les recherches que j'ai pu faire sur ces deux enfants sont extrêmement nombreuseset se sont espacées sur trois ans. Elles s'y ont prêtées avec beaucoup de bonne grâce, sans timidité, ni fou rire; elles ont toujours compris qu'il s'agissait d'une chose sérieuse, et elles étaient persuadées que le moindre erreur pouvait me causer un préjudice des plus graves. Plût au ciel que les adultes qui servent de sujets aux psychologues eussent toujours une attitude aussi bonne!"[3] At the close of the book he says further: "J'ai été aidé dans cette oeuvre difficile (la nature de la pensée) par ces deux enfants qui ne savent pas le premier mot de psychologie."[4] More recently E. R. Jaensch and his students [5] report the observations of children, 11 to 17 years of age, in experimental investigations.

The experimental work about to be described has had an aim somewhat different from those governing the work of Binet and Jaensch. We have been interested in the ability of children to make psychological observations rather than in the study of any bit of experience for itself. Our work has been in the field of visual sensation; but our aim has been, primarily, not to contribute any new data to the child-psychology of visual sensation, but to find out how accurate and reliable descriptions of experiences a child can give in a laboratory under controlled conditions. We have used experiments in visual sensation because this field has been so well worked over that we had furnished a basis for comparison of the observations made by children with those made by adults. Our problem has been largely one of method.

There were eight *Os*: M. (4 yrs. 8 mos.), B. (5 yrs. 6 mos.), K (9 yrs.), J. (10 yrs. 6 mos.), I, (11 yrs.), F. H. (11 yrs.), F. B. (17 yrs. 5 mos.), and A. (20 yrs.).[6] F. B., a freshman in college,

[3] A. Binet, *L'étude expérimentale de l'intelligence*, 1903, 10.

[4] *Ibid.*, 308.

[5] *Zeits. f. Psych.*, 84, 1920, 1ff.; 85, 1920, 37ff.

[6] B. and K. were boys; the others, girls. M. and B. were children whose mothers worked as maids in the college houses and who, therefore, had a poorer home environment than the other *Os*.

and A., a junior, were chosen as older *Os* in order that we might make some comparison of the character of their reports and those of the six children. None of the *Os* had had any previous experience in a psychological laboratory, but A. had taken a one-semester course in introductory psychology.

J. and F. B. came to the laboratory one hour a week for 21 consecutive weeks with the exception of the college vacation periods. The number of weeks devoted to experimental work with the others was shorter, owing to delays in starting or to interruptions. F. H. was unable to come to the laboratory after the first 11 weeks; and the fact that B. was ill for 6 weeks caused an interruption in his observations. We did not start work with A., I., K., and M. until after we had been working with the others for several weeks. With K., especially, the work was crowded and incomplete. M. observed in only 7 parts of the experiment, and her observation-periods were often shorter than an hour.

Because of the age of the *Os* it was necessary to give the instructions in as simple language as possible and to avoid all suggestion. Absolutely no conversation, after the giving of the instructions, took place between *E* and the *Os*. All of the experimental work was done in the psychological laboratory. We endeavored to make the work interesting, and the children apparently liked to come. We found it impossible to get reports on a single phase of an experiment for a whole hour, for the children tired easily. We feel quite definitely that shorter observation-periods would be better; but it seemed impossible to arrange for a number of shorter periods, so that we had to make the most of the one hour a week that the children gave us. To obviate fatigue and loss of interest, we varied our procedure during the hour of observation, securing, for example, reports upon the after-image both in the dark room and outside.

Experiment A

We began the experimental work with a study of the after-image, since here we had an objective check on the experiences of the *O*. It should be noted, however, that this is not an obvious observation, for many psychologically untrained adults fail to observe the after-image even when their attention has been called to the phenomenon. Our instructions carefully avoided all such suggestion.

We attempted to verify the following laws of the after-image. (1) The color of the image is always antagonistic to the color of the stimulus. (2) A contrast-color in the stimulus is effective in the after-image. (3) The after-image is intermittent or periodic, not continuous. (4) The intensity of the after-image is a function of the intensity of the stimulus.

Procedure.—(1) In the verification of Law 1 we used adaptation-cards (18x24 in.) of different shapes: (*a*) oblong, half-black, half-white, with a small circle half-white, half-black, which served as a fixation-point, slightly above the middle of the median line; (*b*) circle, half-white, half-black; (*c*) diamond, half-black, half-white. Each card was exposed to the *O* for 30 sec. The instructions were: "I want you to look at this cardboard about here (a point slightly above the middle of the median line, indicated by *E*) until I put the gray cardboard over it like this. Then look at the cardboard and tell me what you see; tell me all about it."

TABLE I

After-Image Reports

	Without Training					With Training					
Os	No. Trials	A.-I.	Qual.	Period	Mvt.	Weeks Train.	No. Trials	A.-I.	Qual.	Period	Mvt.
M	7	0	0	0	0	—	—	—	—	—	—
B	4	0	0	0	0	26	8	8	6	0	0
K	8	7	7	1	1	9	3	3	3	1	0
J	13	13	13	0	4	23	8	8	8	0	4
I	11	11	11	0	1	12	8	8	8	1	0
FH	13	13	13	0	2	—	—	—	—	—	—
FB	11	11	11	2	0	22	8	8	8	0	0
A	8	8	8	0	1	9	8	8	8	0	0

Results.—The above table indicates the number of trials for every *O*, the number of times an after-image, the correct quality, a periodicity, and a movement were reported spontaneously.

M. and B., the two youngest *Os*, gave no reports, declaring that they "saw nothing." All of the other *Os* described the after-image. It should be remembered that we did not ask for a report on any one aspect of the after-image, so that all descriptions were spontaneously given. Our procedure throughout the work has been to try for a spontaneous report of a given phenomenon and, having obtained it, to ask for it directly in the instructions of the next experiment. We hoped by this procedure to avoid all suggestion.

Samples of the *Os'* reports follow. In view of the purpose of this experiment they seem to the *Es* quite as important as the statistical results. They show the naiveté and the good faith of the *Os*, and give some indication of the difficulties of procedure where so young *Os* are used.

Samples of Reports. The numbers in parentheses indicate the number of the trial for which the report was given.

K. (2) "White on this side and black on that. How did it come through? Saw it, and then when I was telling you I saw it again."

J. (1) "One half of the page on the right side like a white piece of paper and like a little hollow in half of it."

I. (4) "I have the feeling something funny is coming with this. It is a circle like the other one, only turned around."

F. H. (2) "Side that was white is black and side that was black is white."

F. B. (4) "White on right, black on left; center circle white on left, black on right."

A. (7) "Black on this side, half circle of black sticking out; white on that side and half circle."

After an interval of several weeks, the length of which differed with the different *Os*, we repeated this part of the experiment with the same instructions to see the nature of the effect of intervening practice upon the type of report. The results are given in Table 1. Lack of time prevented repetition of this experiment with M, but in the case of B, the next youngest *O*, the period of training in observation produced favorable results.

In order to secure a report of periodicity the following addition to the instructions was made: "Now this time I want you you to raise your hand when what you see is gone." Periodicity was reported by all the *Os*, except M. and B., who "saw" no after-image.

Samples of Reports

J. (1) "Moves, comes back...... now it's gone altogether."

F. H. (3) "It's all gone...... no, it isn't."

(2) When the *Os* had shown their ability to name colors, the second part of the experiment was conducted in the dark room, in order that there might be less opportunity for distraction. In a square opening 3x3 in. in the end of an oblong-shaped box, were placed in turn red, yellow and green glasses. A square of white ground-glass was placed behind the colored glass, and an electric light was inserted into the back of the box through a small opening in the top. In a few experiments outlined apple and diamond shapes were placed behind the colored glasses. The exposure-time was 30 sec. The instructions were: "I'm going to show you a color and I want you to look at it until I put the card over it. Then look at the card and tell me what you see; tell me all about it." The number of trials in each case was determined by the nature of the report.

TABLE II

	No. of Trials			After-Image			Quality			Contrast-Effect		
	R	Y	Gn	R	Y	Gn	R	Y	Gn	R	Y	Gn
M	14	12	10	1	1	1	1	1	1	1	0	0
B	14	12	9	9	6	5	6	3	0	3	0	1
K	7	5	6	7	4	5	7	4	5	1	1	0
J	5	5	6	5	5	5	5	5	5	2	5	5
I	5	11	7	5	10	7	5	1	2	0	1	1
FH	2	2	4	2	2	4	2	2	4	0	0	0
FB	4	4	6	4	4	6	4	4	6	0	0	1
A	3	3	3	3	3	3	3	3	3	1	2	0

Results.—As in Table I, we have given the number of trials for every *O*, the number of times an after-image and the correct quality were reported, and in addition the description of a contrast-effect in the stimulus or in the after-image. For the first 6 trials with each stimulus color M gave no report. After this, with 3 exceptions noted above, she always reported "black." This "black" may have been the brightness-aspect of the after-image or merely the blackness of the dark room; the former seems the more probable hypothesis, since when M "saw" nothing, she gave no report at all. B. showed considerable improvement and the other *Os*, as the samples of reports will show, gave more accurate descriptions.

Samples of Reports

M. (7) "Blue and black." (Stim. yellow.)

B. (7) "Don't see nuthin, don't see nuthin', don't see nuthin'. I see green, green, green." (Stim. red.)

K. (3) "Green. I close my eyes and I see green." (Stim. red.)

J. (2) "Kind of a green square with a red line around it, moving around." (Stim. red.)

I. (2) "Biggest green square, keeps coming and going like a flashlight. When I took my eyes off of the red everything went green." (Stim. red.)

F. H. (1) "Oh, oh! I see green with light yellow around it, pretty." (Stim. red.)

F. B. (4) "I see at first lightish blue-green and then it changes into dark green. Outside of it is a circle of pale yellowish light, then just blue green square, then nothing." (Stim. red.)

A. (1) "Purple, sort of bluish-purple, with yellow haze around it...... it gets darker." (Stim. yellow.)

(3) After several trials with every stimulus the additional instruction was given: "Now this time I want you to say 'go' when what you see has gone. Say 'come' when it come sand 'go' when it has gone."

Results.—All of the *Os* except M, who did not observe in this part of the experiment, reported not only the first and the last appearances of the after-image, but noted several reappearances as well.

Now that the *Os* had spontaneously noted the periodicity of the after-image, the instructions were: "You saw that the color went and then came back. Now this time I want to know how many times the color comes and goes. When you first see it say 'come' and if it goes say 'go,' and so on. In short, every time you see the color say 'come' and every time it goes say 'go'." There were at least 3 exposures of the red stimulus and 3 of the green.

Results.—All of the *Os* (except M) reported recurrences of the after-image, varying in number from one to seven. The two older *Os* were more regular in their reports of periodicity, but otherwise there was little difference between the reports of the older and the younger *Os*.

(4) We asked next for a description of the after-image with eyes closed. The instructions were given as in A 2, with this change; instead of: "Then look at the card and tell me what you see," the *Os* were told: "Then shut your eyes and tell me what you see."

Results.—All of the *Os* described correctly the quality of the after-image with the exception of the after-image with the yellow stimulus. B. called this "black", and I. gave no report whatsoever. It is worthy of note that the *Os* now report not only quality but also shape, and frequently contrast-effect, in the after-image. The report given by B., cited below, is the first of a number of this kind.

Samples of Reports

B. (1) "Green mark, yellow mark, green tree, green house, green river, green sidewalk, people swimming around the edge." (Stim. red.)

K. (1) "I know what it's going to be......see it on the floor now. Square of blue." (Stim. yellow.)

J. (1) "Purple square with green around it." (Stim. green.)

I. (1) "I see something funny......a blue square and a green square." (Stim. red.)

F. H. (1) "Cerise-purple, it's dandy." (Stim. green.)

F. B. (1) "Purple square with pale circle of light outside." (Stim. green.)

A. (1) "Bluish-purple square with light yellow haze around it. Doesn't come as quickly with eyes shut as with the card." (Stim. yellow.)

(5) In order to obtain a description of contrast-effect in both stimulus and after-image the stimuli were exposed for 45 sec. each. The instructions were: "Tell me what you're looking at; tell me all about it"; and when the *O* had reported contrast in the stimulus, "Just tell me about the color." Then followed the after-image contrast instructions: "When I put the card over this I want you to tell me just about the color you see."

Results.—This proved to be a difficult observation to make. We were, perhaps, over-cautious in the phrasing of the instructions, which certainly gave the *Os* no clue as to what we were after, and which may have developed a set against a report of contrast because of similarity to the instructions used in A 1 and 2. However, all of the *Os* (B's report is questionable; but it seems probable that he observed a contrast-effect in the stimulus) described a contrast-effect in the stimulus; but only one of the *Os*, J., always observed a contrast-effect in the after-image, though F. H. described it once out of twelve trials. J. was the best of the younger *Os*. She came the most regularly to the laboratory, so that she had more practice than the others.

Samples of Reports

B. (4) "An automobile with green." (Stim. red.)

K. (1) "Blue right under it." (Stim. yellow.)

J. (1) "Dark around square, green around it." (Stim. red.)

I. (1) "Dark spot in the middle, light green off to the side." (Stim. red.)

F. H. (1) "Green getting around it; red with green rim around it." (Stim. red.)

F. B. (1) "Blue-purple around outside." (Stim. green.)

A. (5) "Green square. Right now it is getting lighter in color, more yellowish, except that now I see the negative after-image of dark reddish purple covering part of it, and that makes the center look like dark blue-green. It seemed to be getting darker and then all of a sudden I realized that it was a darker square getting over it." (Stim. green.)

(6) To obtain a description of differences in intensity, we covered half of each of the glass squares with tissue paper, so that the one half of the square appeared much brighter than the other. The instructions were: "I want you to look at this until I put the card over it; then look at the card and tell me what you see; tell me all about it." The exposure time was 30 sec.

Results.—B., K. and I., after several trials, gave no reports of differences in brightness. J., F. H., F. B., and A. gave satisfactory reports.

Samples of Reports

J. (1) "Kind of purplish; half light purple and half blue, yellow around it." (Stim. yellow.)

F. H. (1) "Where green was, it's purple; where it had white paper on it, very light." (Stim. green.)

F. B. (4) "Pale green on the left, darker blue-green on the right." (Stim. red.)

A. (1) "Purple on one side......can't describe the shade on the other ......it's more brownish and lighter." (Stim. green.)

(7) During the course of the experimentation in the dark room, we asked the *Os* to describe the after-image from an electric light. The instructions were: "I'm going to pull the light on and off three times. When I say 'now' I want you to tell me what you see in the darkness; tell me how the light looks in the darkness."

Results.—All of the *Os* described both the positive and negative after-images, though the reports varied in exactness and in the number of colors described. The youngest *O*, M., in the twenty-third trial gave the following sequence of colors: "Grey, yellow, red, blue, green." Samples of reports from the other *Os* follow.

Samples of Reports

J. (4) "Yellow strings, then lavender, then yellow."

B. (1) "Red coming down, red coming down, red coming down, yet down like a fire."

K. (3) "Those little wires I see...... a little green over those wires."

I. (1) "Big round circle, light in the middle of it, like a lamp hanging up high."

A. (6) "Yellow, shape of bulb......I think I see some blue."

At the conclusion of the whole of our experimental work we repeated this part of the experiment. The reports were all more detailed, *i. e.*, more colors were observed.

J. (3) "The wires are first red, then they turned lavender and then purple, and then black."

(8) This part of the experiment we conducted under ordinary daylight conditions. A brief explanation of the term "after-image" was given to the *Os*.

(*a*) Small red, green, yellow and blue squares of paper about 4x4 in. were fastened on gray backgrounds. Each was exposed for 30 sec. The instructions were: "I want you to look at the color until I put the card (gray) over it; then look at the card and tell me all about the after-image."

Results.—All of the *Os*, except M., who failed to observe the negative after-image, described correctly the quality of the after-image, and reported spontaneously stimulus-contrast, movement, and periodicity. The older *Os* named the colors of the negative after-image more accurately, as, for example, "bluish-purple," "turquoise-blue," etc.

(*b*) Small green, blue, and red squares of paper were fastened on gray backgrounds. Blue, red, and green backgrounds were used for the projection of the after-image. The instructions were the same as in (*a*).

TABLE III

	Stim.	Bkgd.	Stim.	Bkgd.	Stim.	Bkgd.	Stim.	Bkgd.
	Green	Blue	Blue	Red	Blue	Green	Red	Green
O	Trial	Report	Trial	Report	Trial	Report	Trial	Report
M	—	—	—	—	—	—	—	—
B	1	blue*	1	yel., red	1	—	1	green, blue*
	2	blue, green	2	yel. marks	2	green*	2	—
	3	blue	3	blue, red	3	yel., blue, red		
			4	blue, red	4	yel., green		
K	1	purple†	1	purple†	1	yellow†	1	blue
			2	orange†			2	blue
J	1	purple*	1	yellow*	1	yellow*	1	—
			2	yellow	2	yellow*‡	2	green*
I	1	purple*	1	scarlet	1	green*†	1	lt. green
			2	yellow†				
			3	scarlet*				
FH	1	cerise	1	orange	1	yel. orange	1	dk. green
					2	orange	2	wh. blue
					3	yellow		
FB	1	purple*	1	orange	1	yellow	1	blue-green
					2	strong yellow		
A	1	purple*†	1	orange*†	1	greenish-yel.*	1	blue-green

*—shape reported
†—periodicity reported
‡—movement reported

Results.—M. observed no after-images. B.'s reports are again questionable. In trial 2 with the green stimulus and the blue background he may, for example, have seen the "correct" purple, which he called "blue," and his "green" may have been the contrast-effect from that. This is mere conjecture, however, and as such worthless. The case cited shows one of our difficulties with the younger *Os*, a just interpretation of reports. With the blue stimulus and the red background in trial 1 his

report of "yellow" is correct; but in the following trials with the same stimulus and the same background there are no similar reports. This variability is another characteristic of young *Os*.

(9) In order to determine the effect of training and also to test the possibility of using written instructions, in a final synthetic experiment we used either purple or blue colored glass shown to the *O* in a manner similar to that described under (2).

(*a*) Before each of the three 45 sec. exposures the following instructions were read: "I'm going to show you a color and I want you to look at it until I put the card over it; then look at the card. When the after-image comes, watch it until it has gone for good, and then tell me all that you can about it. Tell me all the different things that you've been telling me during the last few weeks, but be sure not to tell me any of these things until the after-image has gone for good."

Samples of Reports

B. (6) "Green piece of grass and green lettuce and a white boat and a red boat and that's all." (Stim. purple.)

I. (1) "Green square and it stayed in the middle of the board. When there's purple there's green around it, and when there's green there's purple around it." (Stim. purple.)

F. B. (3) "Pale circle of bluish light with muddy brown yellow. It comes and goes and it decreases each time in brilliancy." (Stim. blue.)

(*b*) At least two more 45 sec. exposures were made, but this time before each the *Os* were asked to read the following typed instructions. (B. could not read the typed instructions so that *E* read them to him.) "I am going to show you a color and I want you to look at it until I put a card over it. Then I want you to look at the card. When the after-image comes, watch it until it has gone for good, then tell me all that you can about:— (1) the stimulus, its color; (2) the after-image, its color, its size and shape, its coming and going."

Samples of Reports

B. "The stimulus is pink. The after-image is green. It comes and goes, is square."

K. "The stimulus is purple and the after-image green. It came and went and was the same size and shape."

I. "On the stimulus is a little light circle. The after-image is a light green that comes and goes. It is a little bit smaller than the stimulus. It was square."

J. "The stimulus was purple. The after-image is a sort of white cloud. I think it went twice."

F. H. "The stimulus was blue, the after-image yellow. It was the same size and shape as the stimulus. It came and went twice."

F. B. "The stimulus was a purplish-blue, the after-image came and went about three times. It stayed longer the first two times. It was a square about the same size and the square was a muddy brown yellow surrounded by a light pale bluish color."

A. "The stimulus was a reddish lavender. It got darker but that was the negative after-image which was not quite on the square but overlapped

it. The after-image came almost immediately; a dark olive-green square the same size as the stimulus. It gradually got darker and faded into the background."

The reports indicate a great improvement over the earlier ones, and show that even very young *O*s can report on a number of details and that written instructions can be successfully used.

(10) Our final experiment in this group was an attempt to see if these young *O*s could compare two after-images with respect to brightness. One 30 sec. exposure was made with the plain blue glass. When the after-image from that had disappeared, another 30 sec. exposure was made of the blue glass covered with thin white tissue paper. The instructions were: "I am going to let you see two after-images, one after the other. Try to remember how the first one looks and, after you have seen the second and it has gone for good, tell me how the two are different from each other."

Results.—This differs from any of the former procedures in that a report can not be "read off" from the immediately present experience. B. and I. reported no difference in brightness, but all the other *O*s noticed and described the change in this aspect.

Summary of Results and Conclusions

(1) All of the *O*s reported the presence of the after-image, positive and negative, and described correctly its quality, at some time during the experiment. With the exception of M., the youngest, all described the intermittent character of the after-image and a contrast-effect in the stimulus. The relation between the duration of the stimulus and the duration of the after-image was observed by the older *O*s, I., F. H., and A.; the effect of contrast in the after-image by J. and F. H.; differences in brightness by J., F. H., F. B., and A.

(2) The *O*s improve with training. B. and M., the youngest, "saw" no after-image at first, but after training described it correctly. B. gave no reports of any sort during the first 2 hours that he spent in the laboratory, so that we felt that we had reached the lower age limit in the *O* of five and one half years. We decided, however, to "try again," and in the next observation hour B. gave accurate reports, and so improved with training that he frequently matched the older *O*s. We had much the same sort of experience with M. She was still younger than B., but like B., lacked the better training and home environment that the other *O*s had. We worked with M. for only 6 hours, and her reports are inaccurate. However, they show a progressive improvement, and point to the conclusion that the ability to describe one's experiences is a matter of training rather than of age, except so far as the understanding and use of language are correlated with age.

The older *Os* improve with training in accuracy of report and in the number of details they give. J. was the best of the younger *Os*, but she came the most regularly and for the longest period of time to the laboratory. Training, then, is very important. It involves not only improvement in the use and understanding of the language of description, but also an adaptation to the atmosphere and requirements of laboratory work.

(3) The greatest difference between the younger *Os* and the older is the lack of stability of judgment on the part of the younger ones; that is, when an *O* gave a correct report, this was no guarantee that he would continue to do so. A careful description was quite likely to be followed by one, or several, which seemed to have nothing to do with the experience; or by no report at all. This was especially true of M. and B. and, to some extent, of K. and I., though their stability of judgment improved with training. It seems probable that this variability shown by the youngest *Os* was correlated to some extent, at least, with fatigue and ennui. Shorter observation periods might not eliminate it, but they would help to decrease it. J. and F. H. differed very little in this respect from the older *Os*, F. B. and A. Indeed, toward the close of the experimental work, we hardly looked upon J. as a "younger *O*". K. and I. had so much shorter periods of training than B., J., and F. H. that their reports are not strictly comparable.

(4) The reader will have noticed the naiveté and, in some cases, the crudity of the reports. The younger *Os* obviously know only the more simple color-names. We have not had many difficulties in interpreting the reports. They are more comprehensible because of their frankness than many reports of older *Os* in other experimental work. B.'s reports are an exception to this statement. His descriptions of "grass," "lettuce," and "boats" are not easily interpreted. How much of the imagery is based on directly preceding sensory conditions and how much of it is supplementary imagery cannot be said. *E* at no time criticized B. for this type of report or commented on it, except by saying at the beginning of the experiment: "Now tell me *just* what you see."

Experiment B

Since the *Os* had improved with training, we continued our work in the field of visual sensation and took up next a study of *adaptation*. Observations in this field proved difficult for young *Os* because of the constant fixation of considerable duration (for children) which is necessary, and the slowness with which adaptation takes place, resulting in discouragement and monotony. We spent only from 2 to 3 observation-periods on this part of the experiment, so that the *Os* had comparatively little

training. Several of the children complained that the work hurt their eyes, so that it seemed wise not to give more time to it.

Procedure.—(1) The *O* was seated facing a large window. Directly in front of him was placed a square of green glass, arranged in such a way that all lateral light was excluded. The instructions were: (*a*) "I want you to look through the green glass and tell me about the color of things. Keep looking and tell me how the color changes. (*b*) When you look away, tell me how the things which you see look."

Samples of Reports

M. (2*a*) "The sky is green, the road green, everything green and green and green" (at end of 3 min.).
(2*b*) "Everything is white,......no, red."
B. (1*a*) "Everything is green......white......the church is pink......road green......getting black" (at end of 2 min. 20 sec.).
(1*b*) "When you look off things look pink."
K. (1*a*) "Everything is just green......darker green......lighter green" (at end of 3 min.).
(1*b*) "Pink......oh, awful pink."
J. (1*a*) "The sky is green, the automobiles look green, the church green," etc. (at end of 6 min.).
(1*b*) "Things look sort of pinkish......you look sort of pink......whiter than you did."
I. (1*a*) "Green......everything yellow......now green again......the church is a horrid olive-green color......the trees purplish, the sky bright green" (at end of 2 min. 30 sec.).
(1*b*) "Everything bright red......American Beauty color."
F. B. (2*a*) "Green......grayish-yellowish green......gets grayer" (at end of 45 sec.).
(2*b*) "Pink at first, changes back quickly."
A. (1*a*) "Everything is green......everything a little duller.......doesn't really look green to me now......I don't know what color, just natural......a little grayer......even the grass looks gray" (at end of 2 min. 20 sec.).
(1*b*) "Red! Just as red as it can be."

(2) In order to obtain a report on adaptation to light we used the following instructions: "I'm going to take you into the dark room and we are going to stay there for a short time. (*a*) When we first go in tell me how it looks and, as we stay in there, tell me how it changes. (*b*) When we come out into the light, tell me how it looks and how it changes."

Adaptation to darkness was reported by all *O*s except B. M. did not observe in this part of the experiment.

Samples of Reports

B. (*a*) "Dark......green, red......green, blue......and a dog and a boy......dark, red, white......automobile and a blue sky and a stork......hoop and a boy rolling it", etc. (after 11 min.).
(*b*) "All dark......now all white."
K. (*a*) "Black......red, dark......red now, it was pinkish last time......pink......look at my knee......black still" (after 7 min.).
(*b*) No report.

J. (a) "Black......can't see anything......all dark.......trying to see something......all dark......I begin to see a little better......can't hardly see anything though......begin to see things......every minute or so I see a few more things" (after 15 min.).

(b) "Very bright......not so bright."

I. (a) "Can't see anything.......black.......right in front of my eye it's lighter......seems darker than when I first came in......light circles twirling before my eyes......light near the table" (after 8 min.).

(b) "Bright!"

F. B. (a) "Black......little gray spots float around......black......it's browner than it was, not so deep a black......the blackness doesn't hit you as it did when you came in. It is not as hard to sit with your eyes open......looking straight ahead there is grey, black from the sides......white spots and gray mixed" (after 11 min.).

(b) "Everything looks natural."

A. (a) "Just dark......I can't see a thing......little lighter......no objects but gray......perhaps a lighter gray...... a little light streak......quite a bit" (after 17 min.).

(b) "Light, very bright, almost dazzling."

(3) The *O* was seated with his head secured in a head-rest. Five feet in front of him was placed a yellow card with a fixation point. The instructions were: "I want you to sit here with your head in the head rest like this. Then I want you to look at this card about here. As you look at it, tell me what you see, tell me all about it. Keep on looking at it and keep on telling me what you're seeing. (After *O* had reported a change these additional instructions were given.) You saw there was a change in the color. Now just tell me about the changes as they occur." The time of exposure varied according to the nature of the report.

Samples of Reports

B. (2) "Red and a mark and a square mark......gray......a round hoop......white and blue and that's all......looks blue yet."

J. (2) "Yellow......light streaks around the sides, top and bottom......light streaks around the sides or else the middle is darker, I don't know which....almost all the card is a darker yellow, but around the edges it gets lighter."

I. (1) "Funny dark square......a darker yellow except on the edges where it's the color that it really is......much darker......half dark, half light."

F. B. (1) "Yellow......a brighter yellow around the edge and brownish in the center......if anything, it becomes more brown."

A. (1) "Light around the spot (fixation point)......a little darker, the whole thing is a very dirty yellow now......it is almost the same color as the table......I can't tell where it leaves off......very grayish."

Summary of Results.—The *O*s clearly need more training in describing the course of adaptation. Some of the reports, however, are good. In B, 1 and 2, all of the *O*s, except K., noticed the after-effects of adaptation. In B, 3 an especially difficult observation was asked for. The nature of the reports shows that changes due to adaptation were observed, but that the work needs considerable refinement. This part (3) of the experiment was very disagreeable to the children, who grew tired

of "waiting for something to happen" and who could not maintain a steady fixation. All but B. and K., nevertheless, describe some of the effects of adaptation. B.'s tendency to give meaningful descriptions is shown throughout the experiment.

Experiment C

In the next series of experiments we asked for descriptions of contrast-effect.

Procedure.—(1) On backgrounds of red, green, yellow, blue, white and black papers were pasted figures of gray paper. Both were covered by thin white tissue paper. The instructions were: "I want you to tell me the color of each figure as I show it to you."

Results.—We give in full a table of the results of this experiment in order to show how the reports of the different *Os* compare with one another. It will be seen that the older *Os* got the contrast-effect more quickly, that is, with fewer trials, than the younger; though all, even M. and B., made the observation. Observation of contrast-effect was much easier than observation of adaptation, as is, of course, to be expected.

TABLE IV

	Red	Green	Yellow	Blue	White	Black
M	1 white	1 gray	1 gray	1 gray	1 gray	1 gray
	2 "	2 "	2 "-blue	2 "	2 "	2 "
	3 "	3 "	3 blue	3 "	3 "	3 "
	4 "	4 pink	4 "	4 "	4 "	4 "
	5 "	5 "	5 "	5 "	—	—
	6 "	6 "	6 "	6 yellow	—	—
B	1 white	1 gray	1 gray	1 gray	1 brown	1 gray
	2 gray	2 pink	2 "	2 "	2 "	2 "
	3 "	3 gray	3 "	3 "	3 "	3 "
	4 white	5 pink	4 "	4 "	4 "	4 "
	5 gray	4 "	5 "	5 "	5 "	5 "
	6 white	6 "	6 choc.	6 yellow	6 "	6 "
K	1 white	1 reddish	1 brown	1 white	1 brown	1 white
	2 "	2 brown	2 "	2 yellow	2 "	2 "
	3 green	3 red	3 blue	3 "	3 dk. "	3 "
	4 brown	4 "	4 "	4 "	—	—
	5 green	5 "	5 brown	5 "	—	—
J	1 green	1 reddish	1 blue	1 yellow	1 black	1 gray
	2 "	2 pink	2 "	2 "	2 "	2 "
	3 "	3 "	3 "	3 "	3 "	3 "
	4 "	4 "	4 "	4 "	4 dk. gray	4 lt. gray
I	1 gray	1 pink	1 lt. gray	1 dk. gray	1 dk. gray	1 white
	2 "	2 "	2 "	2 "	2 "	2 lt. gray
	3 "	3 "	3 gray	3 gray	3 gray	3 gray
	4 "	4 gray	4 "	4 "	4 "	4 "
	5 "	5 dk. "	5 dk. "	5 lt. "	5 dk. "	5 lt. "
FB	1 blu.-gy	1 purple	1 blu.-gry	1 yel.-gy	1 gray	1 lt. gy
	2 "	2 "	2 "	2 "	2 "	2 "
	3 blu.-wh	1 lav-gy	3 "	3 "	3 "	3 "
	4 blu.-gy					
	5 green					
A	1 gray	1 red-gy	1 bluish	1 yellow	1 dk. gy	1 lt. gy
	2 greenish	2 reddish	2 blu-gy	2 yel-gy	2 "	2 "
	3 gy-gre	3 red-gy	—	3 yellow	3 "	3 "

(2) In this part of the experiment in connection with reports on contrast-effects we secured some data on color-matching. On 6 paper discs (blue, red, yellow, purple, orange, green) were pasted rings of white paper, one-half in. in width. On these rings were pasted sectors of black paper, so that the ring and disc might be equal in brightness. *O* was given a number of small discs (yellow, orange, green, blue, purple, red, black, white). *E* first placed the green paper disc on the electric color-mixer and, having received a description of the contrast-color of the ring, illustrated to *O* how it was possible to combine some of the smaller discs so that when they were rotated on another color-mixer, placed beside the first, the resulting color should match that of the contrasting ring. Following this explanation the different colored discs were in turn put on the mixer. The instructions were: "You saw how I put some of these colors together so that, when the color-mixer was turned on, the resulting color matched the inside ring. Now I want you to do the same. I shall put different colors on the electric mixer, and I want you, by putting some of these colored discs on this mixer, to get colors which will match the inside rings. You may use as many or as few discs as you like."

Results.—With the exception of M. all the *O*s seemed to have had some idea of the proper manner in which to attack the problem. Although M. reported contrast-effects from all colors except purple, she apparently had no idea how she might match the hue and brightness of the ring by combining certain of the colored discs. B. and K. did not always recognize a contrast-color, but they always attempted to match for brightness. They were at first more perplexed by the problem than the older *O*s and were more easily discouraged when good results were not immediately obtainable. K. occasionally departed from the particular problem in his interest in combining various odd colors to observe results. J. always attempted to match for both hue and brightness. She was very persistent in her efforts to secure satisfactory results. I., like B. and K., attacked the problem blindly at first. She always attempted to match for brightness, but she failed to recognize any contrast-color from the purple. F. B. and A., after a careful analysis of contrast-hue and brightness, always attempted to match for both.

The most interesting feature of this experiment was the care and patience shown by the *O*s in matching the contrast-color. Some of the *O*s changed their combinations from 14 to 23 times before they were satisfied with the matches. This result shows that young *O*s can be depended upon for long and care-requiring pieces of work, provided they understand clearly the problem to be solved.

General Conclusions

The experimental work that we have done with children has shown that they can be used as observers in psychological experiments; that they can give accurate and reliable descriptions of their experiences. Training in laboratory procedure is essential, but this is also the case with adult observers. Short observation-periods to avoid fatigue and monotony are desirable, and they should be made as frequent as possible.

The chief difficulties, and these are not unsurmountable, are (1) the use of language that children can understand, (2) the absolute necessity of framing instructions so as to avoid suggestion.

The conditions of this experimental work have been somewhat crude, but they show that with training more elaborately planned experimental work can be carried out. There is no evidence from our work that the children were "unnatural" in the laboratory. Quite to the contrary, once a child had been in the laboratory, its surroundings were taken for granted, and the reports showed no traces of artificiality. Indeed, the observation periods seemed to be enjoyed.

When we compare the children with the two college girls who observed in the experiments, we find that the children show more variability in their attitude and in their reports. This lack of stability decreased with training, but in our work it always had to be dealt with. There was considerable variation from week to week, so that at times it seemed as if the previous week's training had been of no value. There is no doubt, however, but that the interval between observation-periods was too long, and that more frequent observation-periods would favor stability of attitude. On the other hand, the reports of the children were quite as good in most instances as those of the two older *Os*, if we take into consideration the difference in ability to use language. As we have pointed out, the two older *Os* used more exact color names and more carefully chosen words in describing their experiences.

The children, on their side, were more spontaneous in their reports. There seemed always to be more effort on the part of the older *Os* to give what the experimenter "wanted" than on the part of the children, who burst forth into naïve descriptions of what they "saw" without any thought of its being "right" or "wrong." They tended, too, much less toward interpretations of their reports.

As we have said at the beginning of this paper, our purpose was not to make any contributions to the child-psychology of visual sensation. What we have shown, however, is that a psychology of childhood can be built up experimentally. It will take time and patience and some ingenuity, but the results will be worth the effort.

BINAURAL LOCALIZATION OF TONES AS DEPENDENT UPON DIFFERENCES OF PHASE AND INTENSITY[1]

By H. M. Halverson

HISTORICAL SETTING

The reader will find the literature on the binaural localization of sound discussed by Pierce[2] and by Ferree and Collins[3].

In referring to the literature certain terminological usages need to be kept in mind. Hearing or localization is *monaural* if only one ear functions in the process; it is *binaural* if both ears function together. Stumpf's[4] terminology makes a finer distinction. Hearing is *monotic* if only one ear functions in hearing; it is *diotic* if both ears function together in the same manner, *i. e.*, if the same tone or tones are heard by both ears; it is *dichotic* if both ears function simultaneously for different tones, *i. e.*, if different tones or complexes of tones are heard simultaneously by the two ears.

The original problem of binaural hearing has arisen out of the demonstration of the fact that, although there may be monaural localization of sound, binaural localization is more accurate. This fact is brought out in the studies of Angell and Fite,[5] Angell,[6] Starch,[7] and Klemm.[8]

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[1]From the Psychological Laboratory of Clark University. The experiments herein reported were performed subsequently to certain experiments by the writer at the University of Iowa. The monograph reporting these experiments (*Univ. Iowa Studies in Psychol.*, No. 8, 1921) is now in press and explicit reference can not therefore be made to it.

One general way in which the present studies differ from the Iowa experiments is in the appeal made to introspective control of the image of localization and the process of localizing. It is unfortunate that limits of space prevent the printing of complete introspective data, as well as the historical orientation of the problem. All these matters are presented fully, however, in a bound manuscript report of these studies, which is filed under the same title in the Clark University Library, and can be borrowed under the usual courtesies of library exchange.

[2]A. H. Pierce, *Studies in Space Perception*, 1901.

[3]C. E. Ferree and R. Collins, An Experimental Demonstration of the Binaural Ratio as a Factor in Auditory Localization, *Am. J. Psychol.*, 1911, *22*, 250-297.

[4]C. Stumpf, Ueber zusammengesetzte Wellenformen, *Ztsch. f. Psychol.*, 1905, *39*, 276.

[5]J. R. Angell and W. Fite, The Monaural Localization of Sound, *Psychol. Rev.*, 1901, 8, 225-246.

[6]Angell, Further Observations in the Monaural Localization of Sound, *ibid.*, 449-458.

[7]D. Starch, Perimetry of the Localization of Sound, *Psychol. Rev. Monog.*, 1905, No. 28, 1-45; 1908, No. 38, 1-55.

[8]O. Klemm, Untersuchungen über die Localisation von Schallreizen, 2 Mitteil., *Psychol. Studien*, 1913, 8, 497-505.

The further problem has consisted in a determination of the nature of the binaural integration that operates in these finer localizations of binaural hearing. Wilson and Myers[9] have sought to explain this integration as peripheral and occurring by way of bone-conduction of the sound from one ear to the other. Others (*e. g.* Stewart[10]) have sometimes made use of this principle. There have, however, been numerous objections to the assumption of the effectiveness of bone-conduction. The reader should in this connection consult Cross and Goodwin,[11] More,[12] Peterson,[13] and Klemm.[14]

Peterson and Klemm are for this and other reasons forced to a theory of central integration, a type of explanation which is further supported by Watt.[15] The necessity for integration at the center is apparent when it is demonstrated that stimulation of the two ears is readily discriminated: Cross and Goodwin,[16] Peterson,[17] and Baley.[18]

Apart from certain earlier suggestions that localization might be dependent upon tactual sensations of the shell and drum of the ear, upon stimulation of the semi-circular canals, or upon "original special differences of the ears," there have been three principal theories of binaural localization.

(1) In the first place there is the theory that the localization of direction depends upon the relative intensive differences of a sound as it reaches the two ears. This theory seeks to express the angle of directional localization as a function of the binaural intensive ratio. The view is held, with various degrees of generality, by Angell and Fite,[19] Angell,[20] Starch,[21] Wilson and Myers,[22] Ferree and Collins,[23] Stewart and Hovda,[24] and Klemm.[25]

(2) The opponent theory seeks to explain binaural localization as a function of the two sounds in their phase-relations as they are presented

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[9]H. A. Wilson and C. S. Myers, The Influence of Binaural Phase Differences in the Localization of Sound, *Brit. J. Psychol.*, 1908, 2, 362-386.

[10]G. W. Stewart, The Theory of Binaural Beats, *Phys. Rev.*, N. S., 1917, 9, 514-528.

[11]C. R. Cross and H. M. Goodwin, Some Considerations Regarding Helmholtz's Theory of Consonance, *Proc. Am. Acad. Arts & Sci.*, 1891, 27, 1-12.

[12]L. T. More, On the Localization of the Direction of Sound, *Phil. Mag.*, 1909 (6 ser.), 18, 308-319.

[13]J. Peterson, The Nature and Probable Origin of Binaural Beats, *Psychol. Rev.*, 1916, 23, 333-351.

[14]Klemm, *op. cit.*, 4 Mitteil., *Arch. f. d. ges. Psychol.*, 1920, 40, 117-146. Referred to hereafter as "Klemm 4."

[15]H. J. Watt, *The Psychology of Sound*, 1917. A Theory of Binaural Hearing, *Brit. J. Psychol.*, 1920, 11, 163-171.

[16]*Op. cit.*

[17]*Op. cit.*

[18]S. Baley, Versuche über die Lokalisation beim dichotischen Hören, *Ztsch. f. Psychol.*, 1914, 70, 347-372.

[19]*Op cit.*

[20]*Op. cit.*

[21]*Op. cit.*

[22]*Op. cit.*

[23]*Op. cit.*

[24]G. W. Stewart and O. Hovda, The Intensity Factor in Binaural Localization, *Psychol. Rev.*, 1918, 25, 242-251.

[25]Klemm, *op. cit.*, 3 Mitteil., *Arch. f. d. ges. Psychol.*, 1918, 38, 87.

at the two ears: a theory of phase-difference. Rayleigh[26] is the exponent of this theory. He holds that difference of phase is the principal condition of localization for the lower pitches. More and Fry[27] have a similar conclusion. Bowlker[28] reports multiple localizations as a function of phase-difference, Stewart[29] appeals to phase-difference as the principal factor in localization. Hartley[30] works out the mathematical relation between localization and phase-difference.

It would seem in general that psychologists have tended to turn to intensity and the physicists to phase-difference as the primary condition of localization. The fact that theories of phase-difference ordinarily take more account of the intensive factor than do the intensive theories of phase is due undoubtedly to the greater recency of the theories of phase.

(3) Klemm[31] has recently shown that, in case of a sound of short duration, the relative times of arrival of the sound at the two ears may condition localization. He finds that under the conditions for unitary localization priority of presentation is equivalent to the greater effectiveness secured by greater intensity, and thus seems to reduce both intensity and priority to some common factor of auditory effectiveness. In the writer's opinion it is possible that priority in phase may also ultimately be reduced to the same common denominator.

Binaural Localization of a Tone With Closed Tubes

In this first experiment the usual procedure with closed tubes was carried through in order to verify previous results with our observers and conditions and to obtain introspections with them. The following served as observers: Professor E. G. Boring (*B*), Dr. C. C. Pratt (*C*), Miss M. Bates (*A*), and the writer (*D*), all members of the laboratory group in experimental psychology. Dr. M. Yokoyama acted as experimenter for *D*.

Description of Apparatus and Method of Procedure

Two glass tubes of inside diam. of 5-16 in. were mounted so that each was perpendicular to the outside face of a 512 d. v. electric tuning fork, driven by a master fork of 256 d. v. encased in a padded box. These glass tubes were connected by rubber tubing of the same diam. to stethoscopic binaurals in a second room. In the latter room was a large table upon which a semicircumference of a radius of 2m. was marked off. Pasteboards with appropriate numbers were erected at intervals of 10° on the semicircumference to serve as localizing cues for the *O*, who with the binaurals in his ears was seated at the center of the circle. One of the rubber tubes in the first room was cut in two, and two glass tubes, each 90 cm. in length and one telescopically containing the other, were inserted in the breach, so that the length of this conductor could be reduced or increased by sliding the inner tube in or out of the larger. The larger tube was secured to a table upon

[26] Lord Rayleigh, On Our Perception of Sound Direction, *Phil. Mag.* (6 ser.), 1907, 13, 214-232.

[27] L. T. More and H. S. Fry, On the Appreciation of Phase of Sound Waves, *Phil. Mag.* (6 ser.), 1907, 13, 452-459.

[28] T. J. Bowlker, On the Factors Serving to Determine the Direction of Sound, *Phil. Mag.* (6 ser.), 1908, 15, 318-332.

[29] Stewart, The Function of Intensity and Phase in the Binaural Location of Pure Tones, *Phys. Rev.*, N. S., 1920, 15, 425-445.

[30] R. V. L. Hartley, The Function of Phase Difference in the Binaural Location of Pure Tones, *ibid.*, N. S., 1919, 13, 373-385.

[31] Klemm 4.

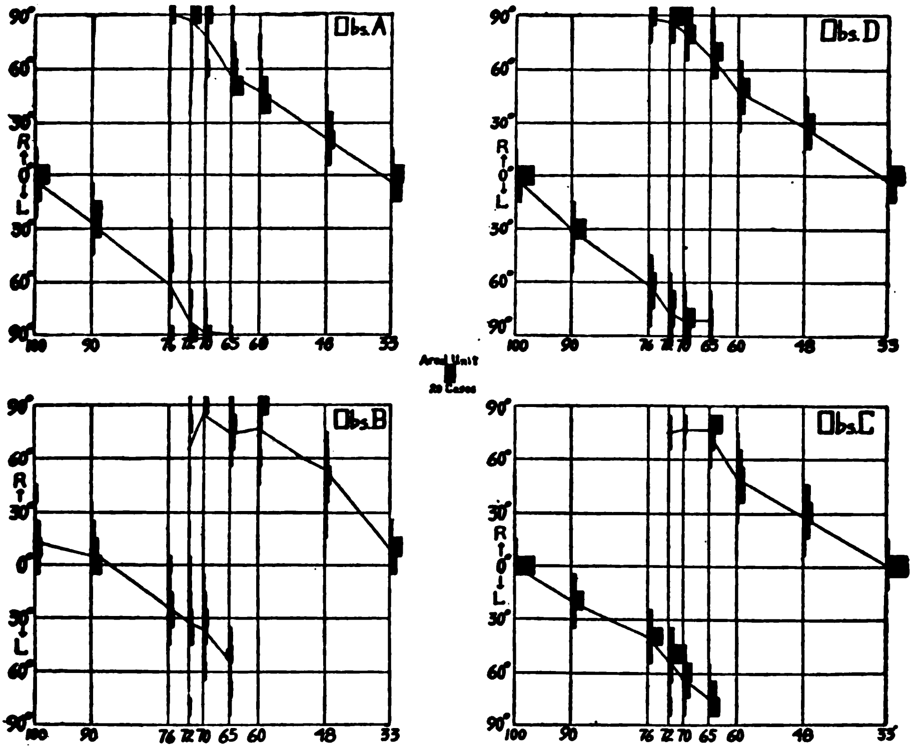

Fig. 1. Closed tube localization. Graphs of four *Os*, showing distribution of frequencies of localizations in degrees right and left of the median plane for the positions indicated on abscissa (cm.). Abscissa values are in terms of the scale of the apparatus (see text) and represent phase-differences of the tone at the two ears. The width of each chart, 67 cm., is the wave-length of the tone, 512 d. v., thus all phase-relations occur once in each chart. The areal unit representing the size of a block of 20 judgments for *A*, *C*, and *D*, should be read as 10 judgments for *B*. Double images occur at positions 76-65.

which a meter stick was fastened parallel with the tube. The open end of the inner tube served to indicate the position which had previously been determined by experimentation. Nine positions, selected because of the peculiar advantages they offered for studying our problem, were used.

With the O in position and the forks sounding, the apparatus was set for one of these positions. The O put the binaurals to his ears, localised the sound, removed the binaurals and wrote his judgment on a paper before him. The stimuli were presented in the order in which the numbers appear in the extension of the numerical value of π ('chance' order).

Results

The general schema for localizing was as follows. The O was told that he would be presented with an auditory stimulus the position of which he was to report in terms of degrees right or left of the median plane of his head. "Zero" represented any position in the median plane, "left 90" and "right 90" stood for positions in the aural axis at the left and right respectively, while intermediate positions were indicated by "left 10", "left 20", *etc.*, or "right 10", "right 20", *etc.*

Graphical Presentation

The quantitative results of the experiment are shown in Fig. 1. Here ordinates are drawn for the nine abscissa values experimented with. The abscissa is recorded in cm. of the scale attached to the telescopic tubes (*v. supra*). Ordinates are degrees right and left of the median plane. Upon each ordinate is erected a histogram showing the frequency of localizations which the O gave for the particular setting of the tubes. The areal scale is shown by the blocks at (70, R50) and (70, R20) in the chart for B, each of which represents a single case. Constancy in localization is indicated inversely by the amount of scatter of the histograms. The diagonal lines connect the averages of the distributions. The direction of the lines indicates the course of the image.

The graphs of the four Os are much alike. Localizations are median at the positions 100 and 33. From 100 to 76 the localizations are at the O's left, from 72 to 65 double localizations occur, and from 60 to 33 the localizations are always at the right. The distance from one median localization to another is approximately 67 cm., which equals at 70° F. one wave-length of the tone used.[22] Double localizations take place one wave-length apart at points midway between the median localizations, a fact which we determined by further experimentation.[23]

A fair indication of the accuracy in localization is shown by the averages of the mean variations of the localizations for the four Os: A, 7.1°; B, 11.5°; C, 5.1°; and D, 5.6°.

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[22] *Cf.* Bowlker, *op. cit.*; Wilson and Myers, *op. cit.*
[23] Bowlker's "two-image" points; *op. cit.*, 323.

B shows a decided tendency to refer the sound to his right. Localizations were frequently reported in the aural axis at his right but it is seldom indeed that they occur beyond 'left 50'.[34] An examination of *B*'s ears revealed that the left orifice is of slightly greater diameter than the right; hence it may be that the manner in which the stethoscopic binaurals fitted into these orifices in some way influenced localization. In testing his ears with an audiometer, no noticeable difference in hearing acuity was found.

Frontal and Lateral Localizations

The graphs show that the position of the image is most accurately determined when the localization is median and is increasingly less accurate as the localizations become more and more lateral. This inaccuracy according to the introspective evidence is due to two facts: failure to distinguish the position of the image both on account of its diffuseness and its vacillating nature, and on account of the failure of the *O*s to make accurate references to their localizing schemata. Since the schemata of our *O*s were visual, it is not surprising that peripheral localizations should have been inaccurate, since the visual field is poorly defined in this region.

In general it may be said that the lateral localizations are more difficult and less immediate than the frontal. The lateral image tends to be more diffuse and vague than the frontal, but is not necessarily so.

For example, *A* always made some such report as the following for the frontal image: "The frontal localization was immediate. I was fixating before me and heard the tone in the exact spot where I was focussing." "I was aware of kinaesthesis of my head giving the impression of left, so I said 'left 90.' This is much more difficult and the judgment is not immediate." Reports for all of *A*'s lateral localizations are similar.

B was less consistent, but always found the lateral localizations the more difficult. He reported once of a frontal localization: "The visual median image is small and intense, but easily localized;" and another time: "The visual median image is vague and of indefinite extent, but easily localized." The typical report for a lateral image describes it as "vague, diffuse and indefinite, and difficult to localize."

C always found the frontal and lateral images equally "clear" and "intense," although the lateral images involved "greater effort due to an inability readily to comprehend the visual schema." For example, *C* once reported as a summary of his experience: "At first the eyes are in a resting position, *i. e.*, looking in front of me, and the visual schema usually includes the area around zero. Now if the stimulus sets up an impression congruent with this kinaesthetic and visual set, the localization is naturally simple. If the impression is not congruent with this set, the visual schema must readjust itself and eye-movement sets in. If this movement is extensive there is awareness of kinaesthetic strains (meaning difficulty), though the actual localization is not difficult."

D, the most practised *O*, could distinguish no differences in the frontal and lateral images, except that the lateral tended to shift more readily under attention.

None of the lateral images compared was in the aural axis. When the image reaches the aural axis it is described by *A* as "more intense auditorily." For *B* the "visual context" was indefinite, "a vague misty blur."

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[34]Hartley, *loc. cit.*, shows mathematically that the image of a 512 v. d. tone should move laterally as far as the aural axis of the observer.

C characterised the tonal image as "very intense," "elliptical," and "scattered." *D* said it was "scrambled, diffuse, piled up with an umbrage of surplus sound making it difficult to analyse."

That lateral localisations are more difficult to judge is Bowlker's view.[35] He states that within 20° on either side of the median plane localisation is very accurate but that beyond 50° it may vary 10° or more.

Introspective Analysis

Localization of sound by means of conducting tubes is usually regarded as a simple process, in which the acoustic image appears to the *O* to be at his front, back or side, and in which he makes his report correspondingly. Psychologically the process of localizing is not simple.

For *A*, who kept her eyes open continually, the process of localising the tone ran itself off in the following manner (analysis of 7 complete reports). (1) Very indefinitely localised perception of the tone began the process in all cases. (2) Then a movement of the eyes, a kinaesthetic pattern, indicating the general direction of the tone (6 cases). (3) Then imaged pressure or kinaesthesis on the head in the general direction of the tone (5 cases), which (4) becomes intense and definite (5 cases). (5) Finally the auditory perception is terminated by its being referred to the visual schema represented by the pattern for orientation laid out upon the table (6 cases).

B first perceives the tone indefinitely localised (analysis of 6 complete accounts). This phase is followed immediately by some portion of the schema for orientation laid out upon the table and by the eye-kinaesthesis involved in following the visual image of the tone along the schema (all cases). The image, as finally placed, was always a visual image with an auditory core, varying from time to time in hue, tint, clearness, and extent. The final judgment consists in the visualisation of the number of the schema, which gives to the tonal image its local context (all cases).

C reports a non-focal visual schema of an arc before him extending on both sides of the median plane just preceding a fairly intensive auditory sensation (analysis of 6 reports). The latter is accompanied by the accrual of a more or less clear visual localizing context to the auditory core. The context consists of an area of the visual schema which gives the general direction of the tone (5 cases). This visual area rapidly narrows down to a clear spot (5 cases) with a definite position in the visual schema. Accurate determination of its position is given in visual directional lines that extend from the spot to the head (3 cases, implied in others). Judgment follows automatically in terms of this visual context. The more lateral localizations (all cases) are accompanied by slight strains of eye-muscles ('effort').

The fore-period for *D* contains a clear visual schema of the table pattern (analysis of 8 cases), with the most lateral portions added eye-kinaesthesis (7 cases). With the presentation of the stimulus the tone is experienced immediately as a spherical visual image (5 cases) which almost instantaneously assumes definiteness in form and position with reference to the visual schema present. Twice when the tone was weak the image seemed to appear only in auditory terms. Accompanying the image is eye-kinaesthesis of focussing on the image (5 cases). The judgment follows at once as the vocimotor image of the number at that particular point of the visual schema to which the image is attached (5 cases). Double images, one at each side, may be accompanied by intensive kinaesthetic imagery of the eyes as of sweeping the visual schema from side to side (2 cases).

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[35] *Op. cit.*, 322.

Two general plans for localizing are thus shown in the introspections. The *O* may first obtain the tone and then apply its image to the visual schema as seen or imaged, or he may visualize the table pattern and see where the tonal image (auditory or visual) makes its appearance on the pattern. The first plan is more generally used, at least in the early stages of practice.

Rough approximations of direction occur usually in terms of eye-movement or eye-kinaesthesis with a given portion of the visual schema in the general direction of the tone standing out more prominently than the rest. Accuracy in direction is obtained when the visual or auditory image becomes attached to the table pattern or the visual schema.

The *Os* show a tendency to close the eyes while localizing, because the principal avenue of distraction is thus shut off, and also because the visual process (most *Os* tend to visualize the acoustic image) becomes clearer when the eyes are closed. *A*, who consistently keeps her eyes open, does not visualize the sound, but experiences it as an imaged pressure or kinaesthesis at a point on the head corresponding with the direction of the sound. Other *Os* at various times have had the same experience.

The visual context of the tone corresponds roughly with its auditory core in its sensory attributes. For example, a weak but clear tone is represented by a small clear visual image and a strong clear tone by a larger image also clear. An unclear tone, weak or strong, calls forth unclear and scrappy visual images.

Double Images at the Critical Phase-Difference of 180°

An interesting phenomenon is the disappearance of the acoustic image at one side and the subsequent appearance of a similar image at the other side, when the phase-difference of the tone at the ears is 180°. It has been suggested that these images are one and the same and that the image must therefore cross through the *O*'s head or behind it.[26]

Good conditions for studying this problem are afforded by this apparatus. The glass slide-tube was moved very slowly from the position 100 to 33 or reverse, changing the phase at the ears gradually so as to insure careful observation at the critical positions preceding, at, and following 180° difference of phase. When the *O* desired, a particular phase-difference was maintained and studied at length. The general results for the four *Os* follow.

A observed two auditory images, one at each side of her head, one less intense at first than the other. They differed in quality and were "two different images." Next "kinaesthesis swept rhythmically from one ear to the other and reversed" its direction. At one time she stated, with an image at the right, "I became conscious of something at my left but on looking there could find nothing. Then for a short time I was unable to tell on which side I was hearing the image, when suddenly it was at my left." Observing later under the suggestion that the image crosses the head, *A* stated that there was a rapid movement of the auditory image across the

[26] Stewart, Phase Relations in the Acoustic Shadow of a Rigid Sphere, *Phys. Rev.*, N. S., 1914, 4, 252; Theory of Binaural Beats, *ibid.*, 1917, 9, 518.

Rayleigh, On Our Perception of Sound Direction, *Phil. Mag.* (6 ser.), 1907, 13, 230.

head during which it "lacked intensity." The image "joined" the *other one*.

B at 180° difference of phase could not localise the image, or else a shifting kinaesthetic attention gave both left and right as correct localizations, or else the sound appeared always to escape to some other place than that which he was fixating. In trying to solve the dilemma he sought at times to place the sound inside his head, but "the localization was equivocal, since it was central and thus at no angle." At other times "the tone at one side would lose its auditory core and leave only the shell of the visual image. Then immediately I realised that the sound was at the other side of my head."

C in all cases imaged the sound at one side as a "large bright spot." There followed "rapid shifts of visual imagery" to the other side of the head "where a second image was also seen." Usually when the image reached the aural axis on one side, "another image appeared concomitantly at the other side. When the two images were equally clear the intensity was greatest. Finally the tone at the first side lost its intensity and its image gradually disappeared." *C* remarked every time that "the images were two distinct images and had nothing to do with each other."

D stated that when one auditory image reached the aural axis at one side it began to gain in intensity, while the sound, now clearly exterior, seemed to "surge into" the ear with the visual context dropping out. Then a slight kinaesthesis in the other ear caused him to look at that side, where he became aware of a "second auditory image weak at first but gradually waxing stronger until both images were of equal intensity. At this point both tones were exterior and surging into the ear. Besides they were diffuse, scatteringly intense, voluminous and unlocalisable except that one was all 'left' and the other all 'right.' Finally the first auditory image weakened and disappeared and only the second image remained, and the visual context began to accrue to it. Kinaesthesis of eye-movement and turning of the head with shifting visual imagery occurred during this entire procedure." At another time *D* stated that "one image originated at one ear, and the other passed away. The sound did not move from ear to ear. It simply accumulated in the images."

When the change of phase at the ears is accomplished *rapidly* an illusory movement of the sound image from one side to the other of the head at 180° phase-difference is evident. This illusory movement is given by the comparatively rapid changes in differences of intensity at the ears and not to a movement of the image itself. It appears to be similar in nature to stroboscopic visual movement.[37]

The evidence points to a rejection of the view that the image crosses through the head or behind it during the period preceding and following 180° difference of phase. It is clear that at 0° difference of phase the image is in the median plane, but no one has ever clearly imaged it in the median plane at 180°. Neither has any one clearly described how it crosses the head at this point. The illusion of movement with rapid changes of phase at this point is, however, decided. The fact that two distinct images may be observed for frequencies of 700 d. v. and above supports the conclusion that the image does not cross the head at 180°. When one of these images moves toward the median

[37]Peterson, *op. cit.*, 350.

plane, the second usually moves a short distance toward the aural axis, and, becoming weaker, finally disappears.[38]

Pseudo-median Localisations

B often reported localisations at 180° difference of phase as median. (See Fig. 1.) He explained, however, that the image was not median in the sense of being localized in the median plane, as is the case at 0° difference of phase, but that the sound was "central" or "at no angle" or "all around" or "equivocal." All the *Os* have experienced a "balancing of the sound," the having of the double sound of equal intensity at the two ears. *B's* reporting this localisation as median simply means that under an instruction for making a single localisation he compromised the double localisation as median. This is a common error which the writer has noticed for years. See p. 186.

Summary

1. The primary psychological factors which enter into the localization of a tone with closed ears, when the phase relations are altered, are (1) a visual, auditory, kinaesthetic, or tactual image of the tone or a combination of these, and (2) a visual or visualized schema of reference, in this case the 'arc-pattern.' The image varies in clearness, extent, and intensity depending upon its position, the intensity of the tonal core and the modality of the image. The 'arc-pattern' varies in clearness and extent with the different observers.

2. The fundamental psychological criteria for the formulation of judgments of position are: eye-movement or eye-kinaesthesis in the general direction of the sound, giving the rough meaning of 'right', 'left', or 'front', followed by adjustments of eye-movement or kinaesthesis in fixating the tonal image with reference to the visual arc-pattern.

3. Lateral localizations are more difficult than frontal because of (1) general inaccuracy in judgments of direction in indirect vision, (2) the diffuseness of the lateral tonal image, and (3) the apparent vacillating character of these images.

4. The tonal image as laterally perceived appears to be auditorily more intense, voluminous and diffuse than when frontally perceived. There is a tendency, allied to its diffuseness, for the visual context entirely to disappear in lateral localization.

5. When the phase-difference at the ears approaches 180°, the tonal image attains its most lateral position and gradually disappears. It is succeeded by a second image at the other side of the head when the difference in phase is again less than 180° and the phase at the latter side is leading. In the intermediate

[38]Hartley says that when "the image has reached a position near 90°, its direction remains unchanged, but in spite of equal intensity, it moves in toward the ear.... Beyond this point there is no corresponding position for an actual source and hence the curves tell us nothing as to where an image is to be expected;" *op. cit.*, 377.

critical position double images, one at each side of the head, may be observed. When phase alters rapidly through this critical position, the rapid succession of images may produce an illusion of movement of a single image through the head or behind it.

6. Observers may, under instructions to make single localizations, compromise a double lateral localization by reporting a median localization.

Binaural Localization of a Tonal Complex by Open Ears: Variation in Position of Dual Source of Sound

Wave-phase may be studied without the use of conductors to the ears. An account of the method and a description of the phenomena involved, as obtained by C. E. Seashore and the writer in 1918 at the State University of Iowa, are being published.[29]

The apparatus then used consisted of two telephone receivers connected in parallel from the same sound-source, with a two-meter stick suspended between them. The receivers were energized so as to produce a tone of 680 d. v. It was found that the *O*, by closing one ear and moving his head carefully from one source to the other with the side of his head parallel to the axis of the receivers, experienced a series of intensive maxima and mimima, which correspond exactly with the points of reenforcement and interference that obtain in the stationary wave set up between the two receivers.

When the *O* with both ears open (the aural axis parallel to the axis of the receivers) moves slowly from one receiver to the other, he localizes the tone in the median plane once for every one-half wave-length that he advances. These points of median localization correspond with the loop centers of the stationary wave.

Midway between the loop centers are the nodes. They are *critical* regions for localization. When the head is moved from a node toward a loop the image of the sound travels in the same direction as the head, but faster, toward the median plane from a position at the side from which the head is moving. The image reaches the median plane when the center of the head reaches a loop center. If the movement of the head is continued, the image then passes on out of the median plane in advance of the head until the next node is reached, when the image lies at the side toward which the head is advancing. Thus, if the head moves from left to right in passing from one node to the next, the image also moves from left to right, starting at the left side, swinging about the head, and ending at the right side. If now the head moves on from this second node to a third node, the image again moves from the left to the right as before.

We may summarize the situation for open-air localization as follows:

1. The space between the receivers may be thought of as divided into unit localization regions, each measuring one-half the wave-length of the tone and every one an exact counterpart of every other.

2. Median and balanced localizations occur alternately for each one-quarter wave-length of the tone used.

3. The movements of the image are experienced, not only in the line of the receivers, but practically anywhere within the range of audibility. These changes in other regions are not everywhere uniform, but at present no definite statement can be made as to their law.

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[29] H. M. Halverson, *Univ. Iowa Studies in Psych.*, No. 8, 1921, monograph in press.

4. With low tones the image of the sound sweeps through a longer arc about the head than it does with high tones. At 512 d. v., *e. g.*, the image sweeps from 90° at the left of the median plane to 90° at the right, whereas at about 1500 d. v. the range of movement becomes so limited that the image is confined to the immediate vicinity of the median plane.

We set up the Iowa apparatus at Clark with the intention of repeating the experiment under more careful control and with psychophysical procedure. Instead of allowing the *O* to move his head, we had him bite on a biting-board and moved the two receivers suspended from the measuring-stick back and forth in the aural axis. A pointer on the biting-board indicated on the cm. scale that connected the receivers the position of the receivers relative to the *O*'s head. The apparatus was set at a definite position and the stimulus presented. When the *O* had localized the tone, the stimulus was shut off and the apparatus then set at another position. Stimuli were presented in chance order.

The tone was obtained, as at Iowa, by placing a small electric generator in the telephone circuit and driving it from a tonoscope used as a constant speed motor. In this way we obtained a frequency of 476 d. v. for which at 70° F. the wave-length is 72.4 cm. Two Edison cells were inserted into the field-magnet circuit of the generator to give the tone the desired intensity.

As soon as we started to work we discovered that the tone was not pure and that different upper partials could be distinguished in it. The second partial, 952 d. v., was very prominent, and in the vicinity of either receiver the more musical *Os* could distinguish still another partial. Attempts to secure purity of tone were unsuccessful, as is apt to be the case in work with telephone receivers.[40] It occurred to us, however, that it would be profitable to study the joint localization of the two more prominent partials, and see whether they operated independently, each in accordance with the law that we had verified at Iowa. Two of our more musical *Os* were able consistently to distinguish two partials, to identify them on a qualitative basis, and to make separate localizations of them. The other two *Os* were less ready at analysis and frequently localized only what appears to have been the more prominent partial. Since the intensity of a tone varies with its localization, that is to say, with the phase-relation of its stimulus, it was to be expected that in these cases sometimes one partial and sometimes the other would be predominant. Moreover, when the two partials lie at approximately the same angle, analysis is comparatively difficult, and the tendency to make a single localization is thus increased.

Ideally we should expect to find the course of each of the two images following the law already laid down. When the receivers are moved continuously with respect to the head, we should expect the two images to pass around the head through the median plane in the direction opposite to the motion of the receivers, and the image of the second partial to complete its cycle twice as often as the image of the first partial.

Except for Baley's dichotic experiments[41] the only instance of localizing with partials, as far as we know, is reported by Thompson[42] who caused the tones of two forks, 256 d. v. and 512 d. v., to be conducted by tubes to the ears so that in the first case the tonal complex was localized in the ears. Then, by causing the 512 d. v. tone to enter the ears in opposite phases, he obtained the higher tone localized at the back of the head while the lower remained localized in the ears.

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[40] *Cf.*, *e. g.*, D. C. Miller, *Science of Musical Sounds*, 1916, 148 ff.

[41] S. Baley, *op. cit.*

[42] S. P. Thompson, Phenomena of Binaural Audition, II, *Phil. Mag.* (5 ser.), 1878, 6, 383.

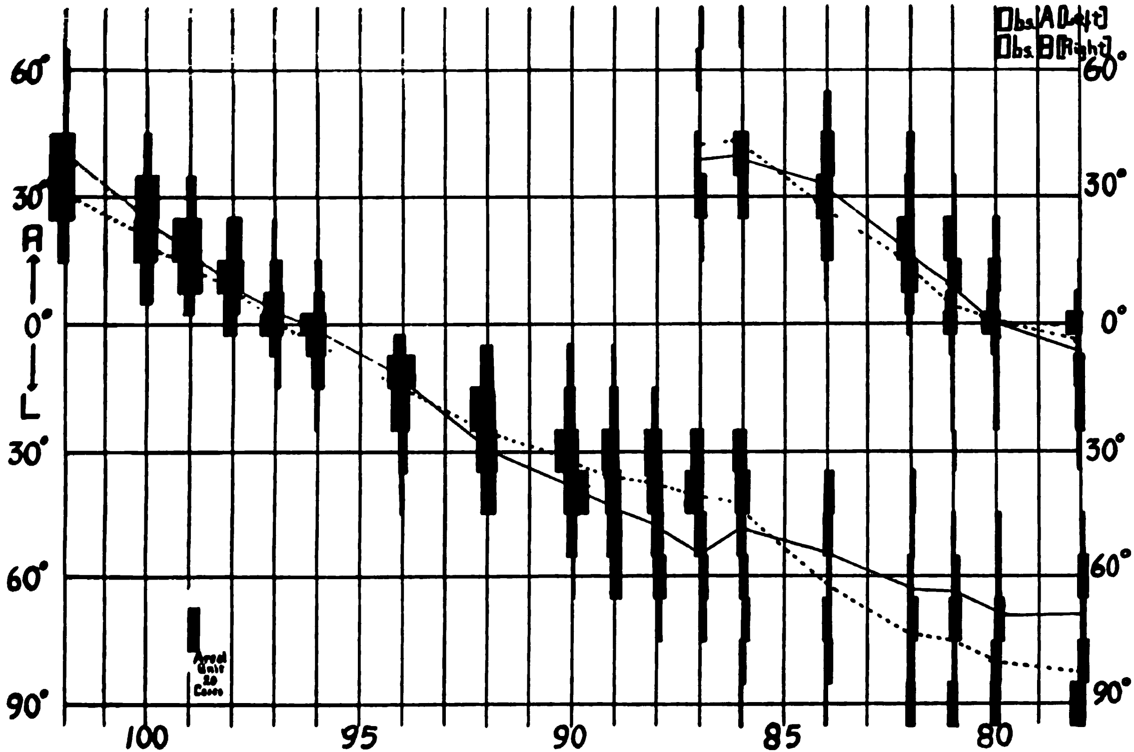

Fig. 2. Open ear localization. Graph showing distribution of frequencies of localizations in degrees right and left of the median plane for positions indicated on the abscissa (cm.). Abscissa values are in terms of the scale of the apparatus for localization with open ears (see text), and represent the position of the *O*'s head between the two sources of tone. Localizations by *A* (dotted line) and *B* (solid line) are indicated to the left and right of the ordinates respectively.

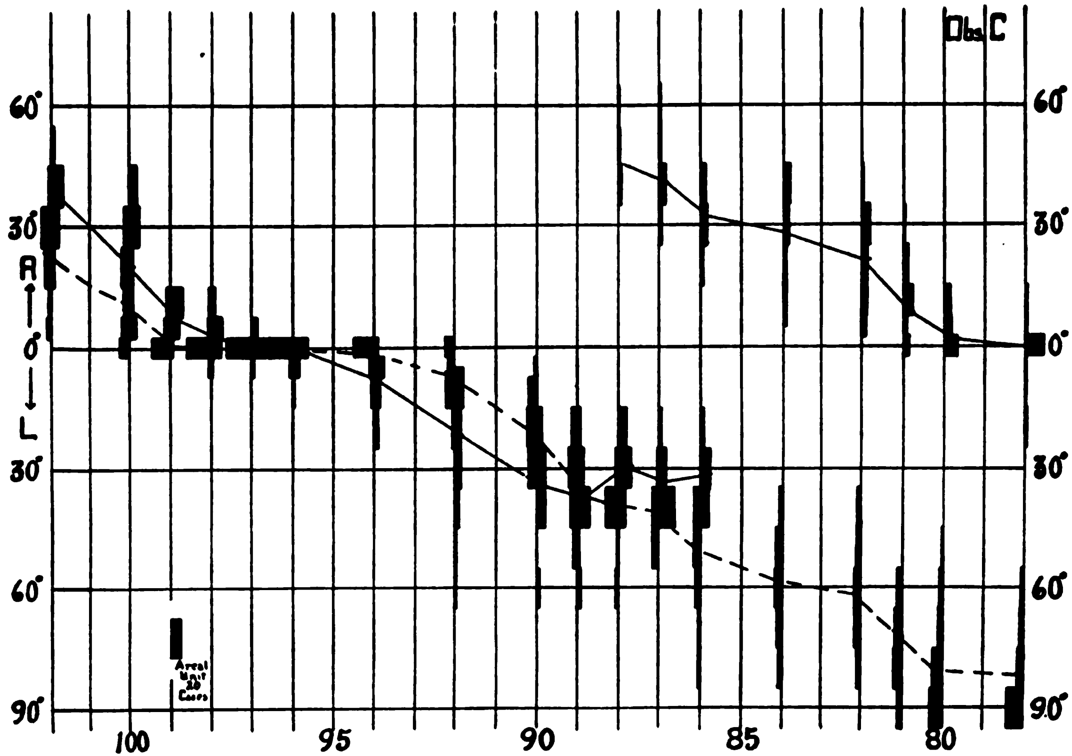

Fig. 3. Open ear localization. Graph showing distribution of frequencies of localizations in degrees right and left of the median plane for positions indicated on the abscissa (cm.). Abscissa values are in terms of the scale of the apparatus for localization with open ears (see text), and represent the position of the *O's* head between the two sources of tone. Localizations by *C* for the 476 d. v. (dotted line) and the 952 d. v. (solid line) partials are indicated to the left and right of the ordinates respectively.

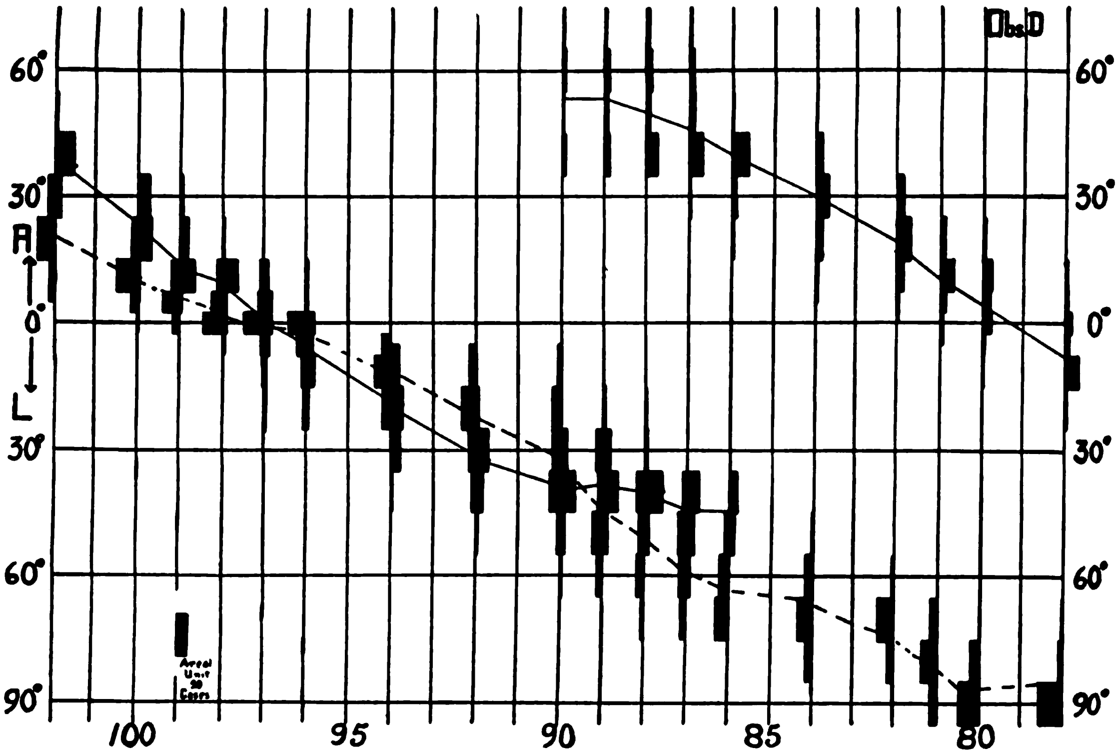

Fig. 4. Open ear localisation. Graph showing distribution of frequencies of localisations in degrees right and left of the median plane for positions indicated on the abscissa (cm.). Abscissa values are in terms of the scale of the apparatus for localisation with open ears (see text), and represent the position of the O's head between the two sources of tone. Localisations by D for the 476 d. v. and the 952 d. v. partials are indicated to the left and right of the ordinates respectively.

Results

Graphical Presentation

The graphs of Figs. 2-4 were compiled from the results of four *Os*. Localizations were made in terms of degrees right or left of the median plane of the *O*. 'Zero' is any position in the median plane and 'left 90' and 'right 90' are in the aural axis at the left and right respectively. The graph of *Os A* and *B* shows the distribution of frequencies in localization in degrees at various positions (cm.) of the sound-sources with respect to the *O*'s head. *A*'s histograms lie on the left side of the vertical lines and *B*'s on the right.

The horizontal line [0°- 0°] represents the median plane of the *O*'s head. Right and left of the median plane are indicated respectively above and below this line. Numbers on the abscissa scale give the positions (cm.) at which the apparatus is brought at rest for purposes of localization.

Each of the ordinates, bearing a histogram, is a graphical representation of the *O*'s localizations at that position for the apparatus. The form of presentation is analogous to that for Fig. 1. Where two separated histograms appear on the same side of an ordinate (see positions 78 to 87), the situation is as follows: (a) either the *O* gave a double localization when the stimulus was presented (frequent occurrence), or (b) he gave one localization at one time and the other at another time under like conditions of observation. Occasionally double localizations were reported at 80 and 89, but these were so infrequent that the graphs fail to show them.

C and *D*, who were more musical *Os* than *B* at least, were better able to analyze the tonal complex into its primary components than were *A* and *B*. Their results are shown in Figs. 3 and 4, where the histogram on the left represents the frequency of localizations of the first partial and the histogram on the right the frequency of localizations of the second partial. It will be seen that both *Os* made double localizations for every presentation of the stimulus and in a great many cases triple localizations (see positions 78 to 88). For example, a typical observation of *C* or *D* at position 86 would be: Upper partial at 'right 40', same upper partial at 'left 40', lower partial at 'left 50'.

The graphs reveal in general that, for positions ranging from 102 to 98, localizations are made to the right of the median plane; from 96 to 88, to the left; and from about 88 to 78, both to the right and to the left. The images cross the median plane at two positions (97 and 80 approximately). The distance between these positions, 17 cm., is one-half wave-length of the upper partial. The course of the more prominent image (the upper partial) is very much the same for all *Os* from 102 to 88, and again from 87 on the right to 78.

Localizations near the median plane are less scattered than at the extreme lateral positions, a fact that corresponds with the assertion of the *Os* that lateral localizations are less definite.[43]

A and *B* never failed to localize the image of the upper partial. In the critical region (87-84) they generally reported two images of the upper partial, one at each side; and from 82 to 78 they often reported an image at the right which was recognized as this upper partial and another at the left which they could not always successfully identify; sometimes they were sure that it was the lower partial and at other times they were unable to distinguish it from the image of the upper partial at the right.

[43] *Cf.* Bowlker, *op. cit.*, 322.

B's localizations are somewhat more to the right of those of the other *Os* (Fig. 2). Other *Os* often reported localizations as far left as 90° at position 78, but *B* seldom reported anything to the left of 70°. Rarely, however, he reported localizations to the right as extreme as 90°, a localization that no other *O* ever made. These reports occurred at positions 102, 89, and 88, but are too few to show in the graph.

The graph for *C* shows that images of both upper and lower partials are reported at each position of the apparatus. From position 102 to 89 both are reported regularly and are introspectively spoken of as "very adjacent" (*cf.* Fig. 3). At 102, 100, 92, and 90, where the images appear to be more definitely separated, the separateness is undoubtedly exaggerated. The images are reported as very close spatially; the anxiety of the *O* to set them apart as two distinct images seems to have caused him artificially to indicate a distance between them. For example, *C* often remarked that in localizing one image, he was also localizing the other as they were in the same lateral position, and yet he stated different localizations for them because of his total impression of their independence. In this region *A* and *B* (Fig. 2) failed utterly to distinguish two images; presumably they localized the two as one. Their failure to distinguish the images accounts for the greater variation of their localizations in this particular region. The images here are very close together and move in the same direction at about the same speed (as other observations show), and this similarity serves to make their perception only the more difficult.

From position 88 to 86 *C* generally reported three images, the upper partial at his right, the upper partial again at his left, and the lower partial at his left (Fig. 3). From 84 to 78 *C* reported always two images, the upper partial at his right and the lower partial at his left.

A feature of *C*'s observations is his tendency to localize toward the median plane. From 99 to 94 and from 80 to 78 the position of the images is predominantly medial. *C*'s introspective reports show that the images in passing from one side of the median plane to the other are localized high above his head, whereas the other *Os* localize the images at points in front of them. It appears thus that for the region considered a given angular change in the position of the image in the horizontal plane of the ears is observed as a somewhat greater arc than a similar angular change in the region of the zenith. *D*, who can at will place the image in front, behind, or overhead, stated that distances above 'seem less' than the same distances in front or behind. He also reported that the distance of the arc 60°-90° left or right is less when observed in the horizontal plane than when observed in the vertical plane of the ears.

D's graph (Fig. 4) is essentially the same as *C*'s. *D*, however, reported more triple localizations than *C* and showed no tendency to localize toward the median plane.

C's and *D*'s graphs show clearly how the images of both the upper and lower partials pass from the right of the median plane to the left. They also show that the image of the lower partial proceeded down to the region of 90, left. The formal experimentation stopped at this point, but incidental investigation showed that for positions beyond 78 this image gradually fades out while another similar to it appears at 90° right and moves toward the median plane. If the image is followed through and beyond the limit at the other end of the graph (102) the same movement is observed in reverse order. When the image of the upper partial has reached a point of about 45° left, it appears in most cases to be "fixed" there, that is to say, it does not move farther to the left as we should expect when the apparatus is moved. Remaining at this place it gets weaker and weaker and finally disappears (at the point where the image of the lower partial is most prominent). Occasionally *C* and *D* reported that the image of the upper partial moved with the image of the lower until it reached a point near 'left 90' when it disappears leaving only the lower partial audible on that side.

C and *D*, under instructions to report what takes place at the angle of 'balanced localization' while the apparatus is being moved, state that the diminution in the intensity and clearness of one image is accompanied by a simultaneous increase of the intensity and clearness of the other image. While the *O* is attending to the very clear image at the left, he finds, as this critical region of localization is approached, that a similar image, of weak intensity and not very clear at first, is claiming attention. Then as the apparatus is moved further along, a position (point of balanced localization) is reached where both images are of equal intensity and clearness; then a point where the second image surpasses the first in intensity and clearness; and finally a stage where the *O* perceives only the second image. The quantitative data (Table I) for these *Os* show clearly this phenomenon.

Bowlker, the first to report multiple images, calls the angle which this appearing image forms with the median plane of the head the "cross-over angle.[44]" This usage, it appears to the writer, is misleading, inasmuch as the image does not cross over from one side to the other. The angle may be called the "angle of balanced localization" and is thus referred to hereinafter by the writer.

From Hartley's calculations[45] for the location of the sound as a function of difference of phase, an angular displacement of the image of 90° right

TABLE I

Number of right and left localizations reported for image of upper partial at various positions indicated. Observers *C* and *D*.

		Observer *C*		Observer *D*	
		Left Image	Right Image	Left Image	Right Image
Position of apparatus with respect to observer's head (cm.).	90	54	2	54	27
	89	54	9	54	39
	88	54	14	52	54
	87	50	26	42	52
	86	46	34	30	52
	84	15	50	10	54

or left of the median plane for a tone of approximately 650 d. v. should occur for a phase-difference of 180°. For tones of somewhat higher frequency, two images should appear whenever the angular distance of an image is slightly less than 90° from the median plane.

Experimentally this conclusion is verified. The image of a tone of 680 d. v. does not attain an angular displacement of 90° before a similar image appears upon the opposite side of the *O*'s head. In fact at this frequency it is just barely possible to make out the two images simultaneously for a very limited portion of the total cycle of phase-differences.

In our work with a 930 d. v. tone double images appeared very distinctly when the *O*'s head was at the point of balanced localization (a quarter wave-length from the point of median localization). Here it was possible to move the head for a small distance either left or right without either of the images disappearing. Hartley's mathematical plot of the direction of the images of a 930 d. v. tone indicates that the angular distance separating the two images should be 102° when both are equally distant from the median plane (p. 381). In the same way he shows that three images, each separated from its neighbor by approximately 50°, should be observed for a tone of 1860 d. v. when one of the images is in the vicinity of the

[44] *Op. cit.*, 323.
[45] *Op. cit.*

TABLE II

Tabular analysis of description of auditory sensations and the corresponding visual images that occur in the simultaneous localization of two partials of a tonal complex under experimental conditions (see text) of localization with open ears. Numbers in parentheses are numbers of instances in which a positive report occurs in the protocols.

Obs.	Auditory Image		Visual Image	
	Upper partial	Lower partial	Upper partial	Lower partial
A	Clear (8), intense (8), thin (3), bright (2), high (10), rough (1), noisy (1), weak (1), unpleasant (2), definite position (5), lacks volume (1).	Clear (3), not clear (2), not definite or intense (2), low (6), varies in clearness (2), ear-kinaesthesis (3), round (1), full (1), voluminous (4).	Clear (2), definite (3), round (2), luminous (1), white spot with black center (1), black spot with yellow halo (7) shading off into grey (1).	Smooth (1), grey (1), and pear-shaped spot (1). Has yellow halo (1), indistinct (2).
B	High pitch (4) of stronger intensity at median plane than at lateral position (1).	Low pitch (4), ear-kinaesthesis accompanies tone (1), unstable (1).	Small (3), a spot (8), definite (5), grey (1), yellow (5), a fuzzy white ball (2) sometimes within a light mist (1).	Clear (2), not clear (2), faint grey spot (2), dark blue spot (4), black (1), just dark (1).
C	Piercing (4), intense (5), hard (6), rough (2), metallic (4), not pure (2), penetrating (7), complex (1), high pitch (18), of oboe or reed timbre (3).	Smooth (6), soft (5), intense (2), not intense (4), diffuse (7), low tone (18), like pure tone of tuning fork (6), more distant than upper partial (9).	Small (4), spherical (9), clear (11) and intense (5), hard (5), bright (4), luminous (3), extensive (1), nearer head than other image always.	Clear (3), not clear (4), small (1), large (3), smooth (1), round (1), oily (1), grey hue (3), dark (1), distant (9).
D	Sharp (9), intense (6), not intense (3), hard (1), definite and clear (11), sparkling (2), unpleasant, (2), impure (2), small (11), high pitch (10).	Soft (7), smooth (4), diffuse (2), extensive (2), thin (1), pure (1), clear (2), definite (1), intensive (2), humming (2), low tone (10), distant (11), fading (2).	Clear (9), intense (6), small (6), spherical (3), definite (8), depthy (2), bright (13), yellow spot (9) with grey halo (3). Like singing flame (1).	Soft (2), extensive (7), unclear (3), thin (3), bluish grey (7), very distant (3), larger than image of upper partial (4).

median plane. Bowlker[46] actually observed three such images with tones of 1675 d. v. and 2090 d. v. At 2310 d. v. he experienced difficulty although the images were present; and at 3050 d. v. he found localization practically impossible. We may therefore say of pure tones that, when the difference of phase at which the tone arrives at the ears is less than a half wave-length, two images are observable, and, when the difference of phase is decreased, a point is reached where it may be possible to observe three images.

Introspective Analysis

Limits of space[47] prevent an even partially complete presentation of the introspective data. The following accounts are of value as general indicators of the nature of the localizing process.

The description implies frequently the ability of the *Os* to distinguish between the two partials. Such distinctions were made by the *Os* partly in terms of the auditory core of the perception and partly in terms of the visual localizing context. Table II summarizes various attributes upon which these distinctions were based, and also throws light upon the qualitative nature and complexity of the perceptions involved.

Observer A begins always with (1) the focal auditory perception of the tone. (2) There follows a period of search, characterized by the kinaesthesis of eye-movement as the visual field is swept. There is a clear visual image of the schema of reference with some of the numbers representing localizations, upon it, and later the visualization of the tone upon the schema. Two tones may be thus simultaneously localized. So far, however, the localization is indefinite. (3) Then comes a rough stabilization of the image in terms of kinaesthesis within the head, giving roughly the direction of the tone; and (4) then the image is localized more accurately by placing it in visual terms upon the visual schema of reference, or rarely by placing the schema upon the tonal image,

Observer B gets (1) first an auditory perception of the tone, which is usually associated with a visualization of a portion of the arc of the schema of localization. (2) Immediately the tone is visualized clearly with definite qualitative characteristics. (3) Then comes a period of adjustment of the visualized tone to the visual schema of localization. Many numbers upon the schema may be clear at first, and drop off as the localization narrows down to a smaller region. The localization finally consists in the reduction of the visualization to the region of a single number. When there is a sudden shift in this localization accompanied by a change in the auditory quality of the core, *B* reports that he is aware of a second partial. (4) There is for *B* a characteristic process of searching, which may precede the localization just described, or may follow it as a verification. *B* sweeps the visual arc with his eyes (in imagery) until he detects the faint image of a tone, then scrutinizes this region to see if the visual image of the tone will become distinct and clear. If it does, he has still to "attach" the image to the schema by the process described above. (5) The process continues indefinitely by way of localizations and verifications, and ends with a verification. It is worth especial mention that, in cases of 'balanced local-

[46] *Op. cit.*, 324-326.

[47] *Cf.* footnote 1. The reader will have to accept the validity of the writer's generalizations or consult the bound manuscript in the Clark University Library.

sation,' *B* definitely reports a double localisation (at the two sides) in the presence of a single auditory quality.

Observer C (1) begins always with an auditory perception of the tone, which is not focal, and a visual image of the arc of localization above the head, which is obscure. (2) Then the arc becomes distinct, the tone becomes focal, and a visual image representing the tone, is added to the auditory core. (3) Frequently this process of clarification is then reversed, and the tonal perception is replaced by the eye-kinaesthesis of searching, an occurrence which means that *C*, having localised the first partial, is now seeking the second. (4) The second partial is next localised in the same manner as the first. (5) Finally both partials are localized together. In this final stage, the two images may be focal simultaneously, or attention may shift repeatedly back and forth from the one to the other. The visualized arc at this stage is apt to become obscure, although *C* experiences no difficulty in making the verbal report of the two localisations.

Observer D (1) in all cases enters upon a localization with a visual image of the arc-path, extending from one side of the head to the other with its ends in the aural axis and making an angle in front about 50° above the horizontal plane. *D* does not, however, visualize the specific position of the schema of reference upon this arc-path. From this point on the process of localization varies with the position of the images.

(*a*) When the lower partial is near the aural axis and the upper partial near the median plane, then (2*a*) the upper partial is localised first, immediately and definitely. The localization is in verbal terms and is not mediated by visual imagery of the schema of reference. (3*a*) Then there is a vague awareness of the lower partial, which is at first located roughly by the eye-kinaesthesis, and (4*a*) is then definitely visualized and localised after the manner of the upper partial. (5*a*) In the final stage *D* verifies his tentative localizations by repeating the process just described.

(*b*) When the upper partial is in the position of 'balanced localization' and is therefore double, (2*b*) the two images of the upper partial appear immediately and simultaneously, and are of like appearance. *D* looks back and forth in imagery from one to the other. (3*b*) Then there is a period of search for the image of the lower partial, which ordinarily appears as faint, indistinct, and diffuse, and sometimes overlaps one of the images of the upper partial. (4*b*) Next the images of the upper partial are applied simultaneously to the visual schema, and (5*b*) then the image of the lower partial. (6*b*) The process concludes with a period of verification as in (5*a*) above.

(*c*) When the images of both partials are both near the median plane, (2*c*) the image of the upper partial is still first placed definitely within the visual schema, and (3*c*) the image of the lower partial, less distinct, is subsequently established. The remainder of the process of localization follows along as in the two cases already described, ending in a period of verification.

Summary

1. Two tones, separated by an octave, were reported by all four observers. Two observers were able to localize each tone of this pair independently of the other at every presentation of the stimulus. The other two observers were able to make this separate localization only at times. *See Figs.* 2, 3, 4.

2. The localizing context for all observers consisted usually in the visual image of a spot, approximately spherical, fixed in a spatial schema at a point from which the tone appeared to issue. This spot varied in clearness, hue, size, distance and other characteristics from time to time.

3. The distance from median localization to 'balanced localization' for the lower tone was approximately twice as great as the corresponding distance for the higher tone. This result is what would occur if the two tones follow independently the law of phase. *See Figs.* 2, 3, 4. For example, the distance that the receivers had to be moved to shift the image of the higher tone from the median plane to the position of 'balanced localization' was found experimentally to be 8.7 cm. This distance should correspond to a quarter of the wave-length of the second partial of 476 d. v., which at 70° F. is 9.0 cm.

4. The image of the second partial is ordinarily visualized as nearer to the observer than the image of the first partial. It is smaller and more distinctly outlined.

5. The image of the first partial shifts through a complete semicircle from 90° to the left of the median plane to 90° to the right of the median plane. The image of the second partial, however, shifts only through an arc of about 90° from 45° to the left to 45° to the right. For it the position of 'balanced localization' is thus at 45° left and right, not at 90°. *See Figs.* 3, 4. This result accords with other investigations which have indicated that with tones of higher pitch the angular range is limited and that the position of 'balanced localization' (the limiting position of movement of the image) is much less than 90° from the median plane.

6. The course of the image in passing through 180° difference of phase (point of 'balanced localization') is discontinuous. The image is either at one side of the observer or the other, or there are simultaneously images at both sides; there is no intermediate localization for the image.

The observers occasionally had the illusion of the movement of the image from one side to the other of the head when the receivers were moved at the critical position of 'balanced localization'. When, however, the stimulus was not moved and the observers were asked to describe the localization at this setting, they usually reported double images, *i. e.*, a reference of the tone to both sides simultaneously. It seems that the tone in this critical position is not ordinarily referred to a single point within the head, but that such a reference comes about only as a result of the tendency of observers to compromise a double localization under an instruction for making a single localization. See pp. 187, 195.

7. When the two images lie together in the region of the median plane it is extremely difficult to distinguish between them, and the observer tends to report a single localization of a tone of different timbre from either of the partials when they are localized separately.

8. The average consistency of localization was greater for the observers who habitually distinguished between the two partials. The average mean variation for these observers is 6.35°. The average mean variation for the other two observers is 10.3°.

Binaural Localization of a Tone by Open Ears: Variation of Relative Intensity of Two Sources of Sound

The fundamental problem of auditory localization is the essential condition of localization. Localization may be due to the difference of phase of the tones as they enter the two ears, to the relative intensities (binaural ratio) of the two tones as they enter the two ears, or to the difference in time at which impulses reach the ear drum (Klemm)[48]. The pendulum seems to be swinging toward difference in phase as the primary condition of localization, although the extent to which other factors contribute is not yet definitely made out. It is not impossible to see how phase-difference might be reducible to time-difference, and the last two theories harmonized. It was the purpose of these experiments to discover to what extent intensive differences co-operate in localization.

Our own experiments with the closed tubes favor a theory of phase-difference,[49] and it seems furthermore that phase-difference is also important in the localizations with open ears.[50] It is plain, however, that differences of intensity may exist at the two ears in open-ear localization, because of the existence of the standing wave with the two ears simultaneously at different regions of it. The actual relations of the intensities at the two ears depend on the relation of the width of the head to the wave-length of the tone. The image is, however, consistently referred to the side at which phase is leading, and this rule holds in many cases in spite of the fact that under it the image is referred toward the side where the intensity of the standing wave is weaker. In a sense, then, the localization may follow a rule of phase-difference even when the rule operates in opposition to the rule of intensive difference.

In incidental observation at Iowa and at Clark, the writer has frequently observed that a very great variation in the relative intensity of the two sources of sound in open-air localization (variation in electric current to the two receivers or in nearness of the receivers to the ears) does not seem to result

[48] See p. 180.

[49] See pp. 187-188.

[50] See pp. 198-200, and the writer's experiments at the University of Iowa, *op. cit.*, monograph in press.

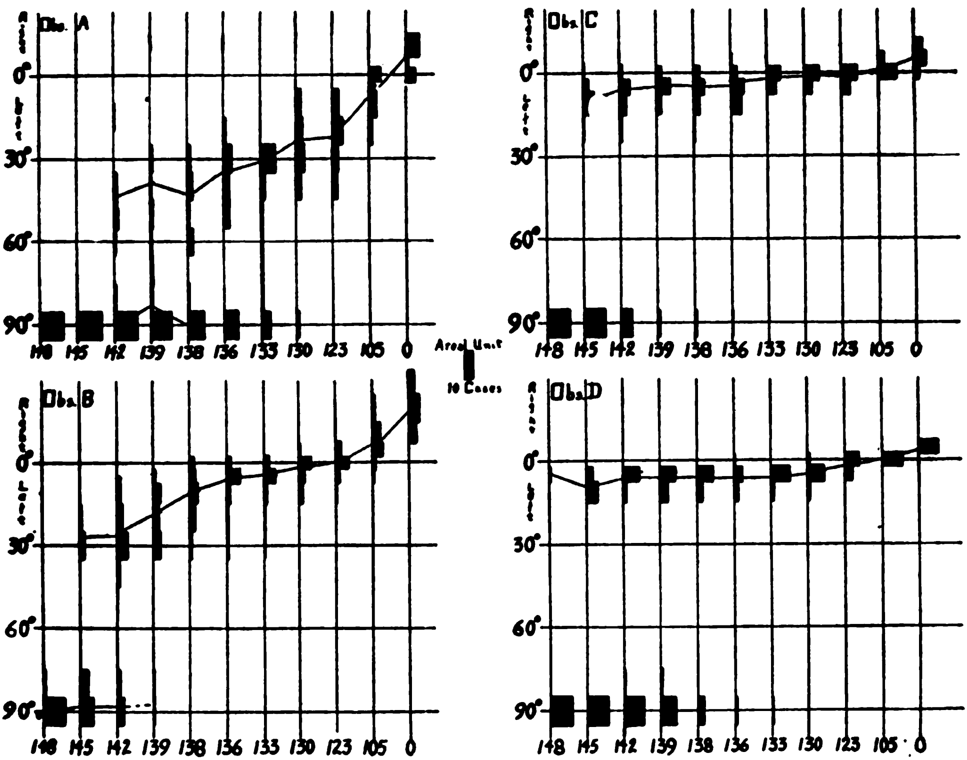

Fig. 5. Open ear localization. Graphs of four *Os*, showing distribution of frequencies of localizations in degrees right and left of the median plane for the positions indicated on the abscissa (mm.). Abscissa values are for the scale of the rheostat, and represent, electrically, successive values of the binaural intensive ratio: "0" indicates equal current to the two receivers on either side of the head, and "148" maximal intensity at the left with minimal intensity at the right. The medial and lateral images, distinguished by the observers, are shown separately on each graph.

in a movement of the tonal image as long as the phase-relations of the two sources remain unaltered. In the following experiments he has sought to investigate this matter systematically.

The same apparatus was used as is described in the preceding section of this Study except that the intensity of the sound at the receivers was reduced by removing the two Edison cells from the field-magnet circuit of the generator. The receivers were brought in toward the *O's* head until they were but one-half wave-length apart. Thus only a few cm. separated the ear from the receivers. By placing the receivers but one-half wave-length apart, the head becomes a prominent barrier to the passage of a tone of this frequency (476 d. v.). There is thus no standing wave to be taken into account.

A slide rheostat with mm. scale (central position, "mm."; each extreme, "152 mm.") was inserted into the circuit of the receivers with each receiver in parallel with one side of the resistance. A shift of the slide therefore decreased the current through one receiver and increased it through the other, resulting in corresponding alterations in the intensity of the sound. At the extreme position of the slide one receiver was short-circuited and the other, in parallel with the entire resistance, maximally energized. In the experiments herein recorded the extreme ends of the rheostat were not resorted to since the receiver ceased to be effective as stimulus before short-circuiting was reached.

The *O* was to localize the sound, using the plan of the previous experiment. The slide was moved to one of the positions of the rheostat indicated in Fig. 5 (mm. scale) and the stimulus presented. After the localization had been reported, the slide was moved to another position and the same procedure followed. The stimuli were presented in the order in which the digits appear in the extension of the numerical value of π ('chance' order).

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The receivers were connected in phase so that when the intensity at the two sides was equal, the sound was then localized approximately in the median plane. (When the receivers are connected in opposite phase, the localization is at once lateral even though the intensity of the sound is equal, 'balanced localization'.)

Positions on the rheostat were selected so that, with the exception of one position (0 mm.) where the electrical intensities are equal, the intensity of the left sound should always be greater than the right. Under these conditions we should expect localization to favor the left side throughout.

The results for each *O* under these conditions are plotted (Fig. 5) from the results of twenty-five observations at each position. The abscissa values on each graph are the millimetric positions on the rheostat at which the slide was set when the stimulus was presented and the ordinate values are degrees of localization right and left of the median plane. The positions investigated are spaced equally along the abscissa, although the intervals between them represent very different amounts of electrical change. These positions were chosen to give a large

number of points at regions critical for change in localization. The significance of the histograms erected upon the ordinates is the same as for the other charts discussed above.

The graphs are similar. The *O*s agree essentially in changes in localization caused by changes in the relative intensity of the sounds at the receivers. All tend to localize the sound toward the side of the greater intensity although they differ somewhat in the amount of this tendency. With the relative intensity at the ears equal (0 mm.) the localization is for all *O*s a trifle to the right of the median plane; the apparatus was apparently not precisely adjusted for median localization. At 105 mm. on the rheostat the localizations have taken a slight turn toward the left,—not a great difference, however, since the slide of the rheostat is moved 105 mm. out of the total possible distance of 152 mm. For each succeeding move of the slide the localizations show more and more a lateral tendency until a position is reached where, in addition to the image so far attended to, a second image, at first faint and elusive, makes its appearance at the *O*'s extreme left. From here on the first image wanes in clearness and intensity, though very slowly, while the second image waxes correspondingly stronger. [For *A* a point (139) is reached where regions of localizations for the two images overlap, although the images in each separate case remain distinct.] Subsequently (148) the first image disappears and the localization is of the second image alone. In a word, then, as the binaural ratio of intensities operates to favor one side more and more, the image moves toward that side until it reaches a point where it disappears. The point varies with the *O*s (8.0°- 43.3°), but in no case does the image approximate the aural axis of the observer. The illusion of a shift to the aural axis comes about because a second image is substituted in the aural axis for the first which disappears above the axis.

It is important to note the large differences required in the intensity-ratio (as shown by the millimetric distances on the rheostat) to effect small changes in localization. The slide of the rheostat was moved more than two-thirds its entire distance to the left (0-105 mm.: an electrical ratio greater than 5:1) to effect the first small change in localization, while no intensity-ratio was adequate to move this image in question laterally more than 22.1° (av.). The position of the second image was not affected by changes in the intensity-ratios. Some of the *O*s were more sensitive to the presence of this second image than others. The degree of sensitivity of the four *O*s to this image is indicated on the individual graphs by the number of cases in which the second image was reported.

Series II: *Discrete Variation of Binaural Ratio with Tones in Phase Throughout Complete Intensive Range*

This group of experiments differs from the first series in that the binaural ratio of the intensities was varied over the complete

intensive range from an extreme position with the right tone more intense to an extreme position with the left tone more intense. With each *O* 25 localizations were made at each of nine positions. These positions were selected in preliminary experiments so that they would yield data at the more critical regions of change and also so that they would offset the constant error of all *Os* to localize toward the right. The median position gave approximately median localization and the other positions localizations to the right and left respectively.

The results are shown in Table III. In each cell of this table the localization indicated at the right is of the frontal image, and at the left of the lateral image. As we have already seen in Series I, the frontal image never fuses with the lateral, and the two exist together in a certain critical region. For *A* and *B* the frontal image never becomes more lateral than 43° right or left of the median plane; for *C* and *D* it moves scarcely at all, never passing beyond 7° from the median plane. The average of the average mean variation of localization for all *Os* is 4.3°. The course of the frontal image is also shown in Fig. 6.

Series III: *Continuous Variation of Binaural Ratio with Tones in Phase throughout Complete Intensive Range*

The *Os* were now asked to note the changes in localization when, with both receivers sounding, the intensive ratio was varied continuously. The variation was accomplished by presenting one binaural stimulus first and then moving the slide of the rheostat. Two procedures were followed. (1) With the intensity of the two receivers approximately equal, the intensity of one was increased while the intensity of the other (necessarily) decreased. (2) With a difference of intensity between the two receivers, the intensity was varied so as to bring them to approximate equality of intensity. The movement of the slide was made by hand as uniformly as possible, and required, after practice, 35 sec.

The results accord with those obtained in the two preceding series of experiments, with one exception. There is reported by three *Os* a movement (which proved to be illusory) of the image moving from the median plane to the aural axis. When the intensities of the receivers are varied gradually from equality to a point where one completely overwhelms the other, the image moves from a position in the *O*'s median plane to a certain point, not far from the median plane (*cf.* Fig. 5), on the side which intensity favors. Here the image remains fixed, while a second image gradually becoming more intense appears at the same side in the aural axis accompanied by 'an appearance of movement' from the position of the first image to the position of the second.

That this movement does not involve the original image itself is shown by the fact that the latter may be observed in its fixed position after the 'movement' has occurred. The phenomenon is, as it were, a withdrawal of support from the first image in favor of the second. Upon reversing the intensive change the 'movement' recurs in the opposite direction.

The following analyses of reports indicate the nature of this 'movement' for the various *Os*. The numbers in parentheses represent the number of instances reported.

Observer A reported 17 times that in passing from the region of the median plane to the aural axis the sound image "splits up." The passage is not a "simple trip", but very complicated. The image in the region of the median plane first begins to fade. Then another image appears at the aural axis and soon becomes clear. While the image at the median plane is still clear, "something" appears to move down toward the image at the aural axis, although the movement is not so much a movement of the image as a "sweep or shift of the visual schema" with respect to the images. The 'movement' takes *A*'s attention from the first image; but after she has looked back she finds the first image still near the median plane (16). Only once was there no appearance at all of movement: one image faded out first and then the other appeared.

Observer B does not in the majority of cases get the 'movement'. In 24 cases the tonal image in the median plane faded out completely while the image in the aural axis was appearing. He reports, however, 11 instances of a phenomenon of 'movement' toward the median plane, when the intensive variation is toward equality of the binaural ratio. These cases are of two kinds. (1) With the image at the aural axis the "sound-mass stretches out," becoming larger and less definitely localized until it reaches the region 30° from the median plane. Here it "pours in," while going out at the aural axis. During this interval, the "tone" stretches from the aural axis to 30° "like a double paddle, with nuclei at these points and an attenuated strand of image between." Then the image at the aural axis fades out while the image at 30° gets "clearer, contracts, and moves slowly toward the median plane" (4 cases in all). (2) The tone starts at the aural axis, and then passes smoothly to 30° *without traversing the intermediate spaces*; then it proceeds slowly to the median plane. *B* was unable to explain how the 30° and the aural axis could thus appear to lie adjacent in his localizing space. He thought that his cue to the 'movement' involves "something more than visual imagery" (7 cases in all.)

Observer C. At first the image moves a little from the median plane (30). Then this image dies out and another appears at the aural axis (30). In the early trials the sound has the appearance of movement from one position to the other, "as though passing through an hour-glass or pipe-system" [*cf.* *B*'s "double paddle" above]; nevertheless the image does not move between these points. Later *C* sees the phenomenon as "a gray screen between 20° and the aural axis in which the image at 20° oozes out slowly while the image at the aural axis grows clearer" (4).

Observer D. The image remains in the median plane for a time and then moves slightly to one side (20). Then sometimes the image, with its accompanying tone, gets gradually weaker, while an unclear, diffuse "sound-mass" collects at the aural axis (13). Rarely in these cases it appears as if "something had slipped" from the frontal region down to the aural axis (2). At other times, however, a "cloud of sound" gathers at the first image on the side toward the aural axis, "stretching out vaguely" toward the axis (7). Subsequently this "cloud draws up" about the image at the aural axis (7). The total phenomenon gives the impression of movement without actual movement.

TABLE III

Average localization with open ears of frontal and lateral images with electrical variation of binaural intensive ratio. Phase approximately equal. The localization of he frontal image is shown at the right of each cell of the table; the lateral image at the left.

Rheostat Scale (mm.)		L148	L142	L136	L130	L105	R20	R80	R110	R130
Electrical intensive ratios		75:1	29:1	18:1	13:1	5.5:1	1:1.3	1:3.2	1:6.2	1:13
Observers	*A*	L90	L89 L43	L88 L35	L76 L28	L10	R8	R84 R22	R75 R30	R89 R36
	B	L82	L80 L32	L9	L7	R8	R70 R29	R81 R36	R80 R40	R90
	C	L90	L86 L6	L85 L6	L72 L5	0	R4	R5	R90 R7	R89 R7
	D	L90	L90 L7	L90 L5	L4	0	R4	R90 R5	R89 R6	R89 R7

TABLE IV

Average localization with open ears of frontal and lateral images with electrical variation of binaural intensive ratio. Left tone leading in phase. The localization of the frontal image is shown at the right of each cell of the table; the lateral image at the left.

Rheostat scale (mm.).		L148	L142	L136	L130	L105	R20	R80	R110	R140
Observers	*A*	L90	L89	L88 L61	L86 L50	L85 L44	L26	L9	R1	R89 R40
	B	L88 L37	L89 L36	L87 L31	L90 L29	L16	R20	R84 R27	R89 R36	R90
	C	L90	L90 L42	L90 L40	L85 L38	L32	L5	L1	R1	R90 R5
	D	L90	L90 L50	L90 L48	L90 L41	L40	L10	R90 0	R90 0	R90 0

Series IV: Discrete Variation of Binaural Ratio with Phase-Difference throughout Complete Intensive Range

In order to obtain some information concerning the relation of the effect of the variation of phase-difference to the effect of the variation of intensive ratio, we repeated the experiments of Series II with the left tone leading in a phase an amount necessary to bring the localization of the image to the region of 'left 30°-40°.' The change of phase was accomplished by shifting the receivers, still one-half wave-length apart, with respect to

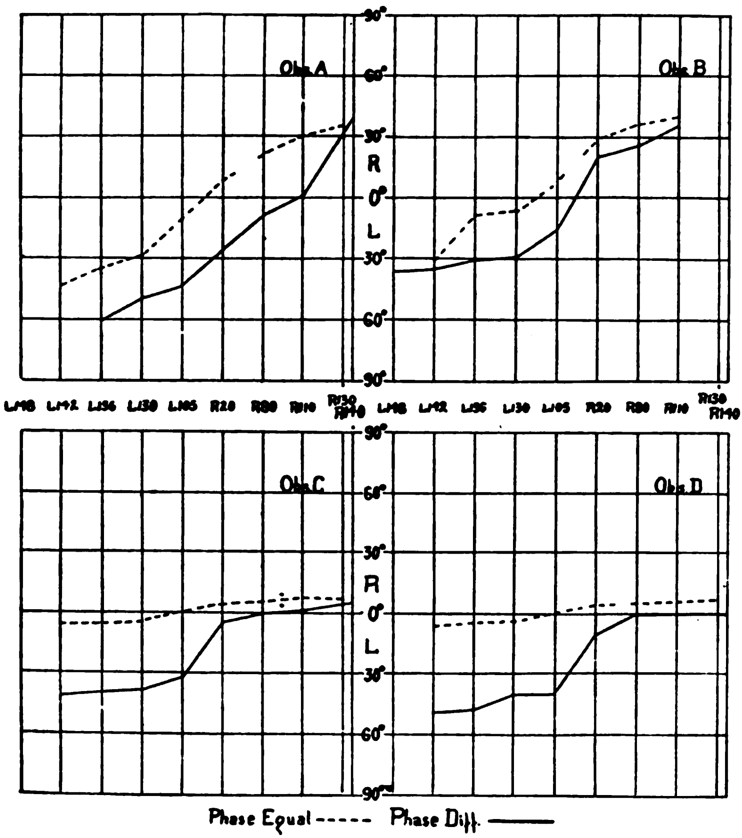

Fig. 6. Open ear localization. Graphs for four *Os*, showing localization (ordinate) as a function of binaural intensive ratio (abscissa). The abscissa values are settings of the rheostat controlling the relative intensity of the two sources of sound. See text and Tables III and IV. The dotted line is for the two tones approximately in phase, and the solid line for the left tone leading in phase.

the *O*'s head. This shift resulted also (presumably) in a slight intensification of the left source of sound, since this receiver was now nearer the head. The intensive difference is, however, to be thought of as having a small effect in comparison to the effect of the displacement of phase.

The observations are only slightly different from those of the three preceding series and do not alter the general conclusions. The frontal image shifts from 'left 47.5°' to 'right 20°,' the average of the four *O*s. It is never confused with the lateral image of the aural axis, but when it has attained its extreme lateral position all the *O*s report a peculiar unexplainable disturbance, as of sound widely distributed in the region separating this image from the image in the aural axis (see the introspective analyses: Series III, p. 205). The numerical results are shown in Table IV. The average of the mean variations for the localizations of all *O*s is only 3.2°.

Fig. 6 shows the effect that the introduction of a displacement of phase has upon the function that localization is of the intensive binaural ratio. The graphs of the figure show for each *O* the average localizations of the frontal image when there is no difference of phase (Series II: Table III) in comparison with the average localizations when the left tone leads in phase (Series IV: Table IV). It will be noted that in general the effect of introducing a phase-difference is simply to shift all localizations for a given binaural ratio toward the side of leading phase. In the cases of *C* and *D*, who show very little variation in localization when the phase-difference is zero, the variation is increased when the left tone leads in phase. This variation occurs on the side toward which there is greater latitude because of the change, *i. e.*, away from the shift due to the change in phase-relation.

The positions of the lateral image are not shown in Fig. 6, because this image always lies close to the aural axis.

The introspections show that where single images are reported the schema for localization for each *O* is that reported in the localization of a pure tone (pp. 187 ff.), and that the schema for double localizations is that described in the localizations of a tonal complex (pp. 198 ff.). In the present case, however, one of the images does not appear to move.

The following introspection by *B* sums up the situation nicely. He gives five other similar reports; and *A*, *C*, and *D* also report similarly although less fully.

"A very difficult localization. The tone at first was visualized indefinitely at my left between 90° and 30°. I do not mean that the visual images stretched from 90° to 30° for each image was small, subtending an arc not over 8°. The trouble was that I could not get the schema of reference to come into the visualization clearly. For a long time I sought (=eye-kinaesthesis of sweeping over the arc) for the place. *E. g.*, first I would

visualize the 90° position clearly. In doing this I could not see the tone! When I got the tone brought into the visual imagery, the imagery of the schema of reference would slip away. I would know that the tone was about 90° (*i. e.*, eye-kinaesthesis the same), but would not feel sure of the localization since the schema of reference was not visually clear. Then I would sweep through a small arc upward and the visual image of the tone would follow. About 30° this visual image would disappear and the visual image of the schema come in. Thus I got to know that the tone was between 90° and 30° without being able to say where. It also appeared as if 90° and 30° were only about 20° apart, since the sweep from one to the other was so small. I kept this chase up for a long time without getting the localization definite. Then I tried hard to see if I could get the visual image of the tone to come in while I had a clear image of 90° on the schema of reference. Soon I succeeded. The tone was distinct at 'left 90'.

While I fixated the tone here I became aware of another tone of the same auditory quality higher up on the schema of reference. I imaged it as in indirect vision. I turned my imaginal eye to it and saw it clearly, smaller than the other tone. At first it was unlocalized (indirect vision), then I got in rapid succession a number of visual localizations of it at about 40° to 30°. Finally the 30° reference became distinct. (This is an inadequate description of a long and very complicated consciousness, which is, however, a consciousness typical of the equivocal judgments that occur in these critical regions.)"

Supplementary Tests of the Rôle of the Binaural Ratio

We have found, when the binaural intensive ratio is altered, that the frontal image is limited in its range of movement, and that the lateral image, when it occurs, maintains an approximately fixed position. When both images are observed 'simultaneously', they may appear to be connected across the intervening space by "attenuated strands" (*B*) or "shooting streaks of gray" (*D*), or the connection may consist entirely in the fluctuation of attention between the two images (*A*, *C*) accompanied by eye-movement. If, with the stimulus sounding, one of these images appears when the other disappears, the *O* may experience a 'movement' from the one position to the other; it is not necessary, however, that this 'movement' should be anything more than a meaning applied to the successive occurrence of the two localizations. Except in this sense, no intensive difference is great enough to bring the frontal image near the aural axis. What happens ordinarily is that the frontal image becomes weaker as the lateral image becomes stronger, and that in this transition the auditory core, to which the visual image is the localizing context, seems to "detach itself slowly and subtly and to become attached" to the lateral image. Such a discrete shift of context may readily give the illusion of a continuous movement of the tone, especially if the localizations are not being made under an introspective *Aufgabe*.

There are some incidental tests which the writer has performed and which support this general conclusion concerning the effect of variation of the intensive ratio upon the localization of the tone. These tests are as follows.

(1) Series III in an abbreviated form was repeated for three *O*s, who, however, held the two telephone receivers close to the ears. The results, with the receivers thus placed, were exactly the same as those reported for Series III, (*q. v.*, p. 240 ff.).

(2) The intensity of the sound in one receiver was varied continuously by inserting into the circuit of one receiver an inductorium and varying the intensity inductively. The results of the localization of the image were again the same as in series III (3 *O*s).

(3) Three *O*s in localizing with closed tubes were asked to pinch one of the rubber tubes leading to the stethoscopic binaurals, after the appar-

atus had been set so that the image was localized in the median plane (phase equal). They all reported that the original image continued in the median plane until it disappeared. Meanwhile a second image gradually appeared near the aural axis. By careful manipulation of the tube these *Os* were able to find a condition (amount of pinching) under which both images were simultaneously observable.

(4) In open-ear localization with telephone receivers the image is always localized in the median plane when the *O*'s head occupies the position that corresponds with one of the maxima of the standing wave set up between the receivers (*q.v.*, p. 188). With a tone of about 700 d.v. and the *O*'s head in the position of one of the maxima, the receivers were moved one-quarter wave-length to one side. In this position the intensities of the two sources are approximately equal at the two ears, since the center of the head is now at a node of the standing wave; and, since the intensities are equal, the localization should be median. The *Os*, however, do not find a median localization, but experience the phenomenon that we have described as a 'balance' of intensities. The tone is heard on both sides at once and the image differs in extent, volume, timbre and intensity from the image of the median localization. If it be objected that this movement of the receivers does alter intensity slightly, since one ear is now nearer its receiver than is the other ear, it may be replied that the difference is inconsiderable in effecting localization, since, if the difference is made even greater by moving the next maximum to the center of the head, the *O* again gets a median localization.

Difference of Phase vs. the Binaural Ratio

We have already seen (p. 200 ff.) that the outstanding problem of binaural localization is the determination of the relative rôles of difference of phase and of difference of intensity in fixing the localization of the tonal image.

Rayleigh,[51] as we have seen, came to the conclusion in 1907 that the localization of low tones is a direct perception of difference of phase, whereas the localization of high tones (above 768 d. v.) is dependent upon intensive differences. Stewart[52] similarly holds that intensity is not to be considered as "an important factor in localization of pure tones" between 256 and 1024 d. v. Stewart found that when the intensity at one ear was diminished the image, originally in the median plane (equal intensity and no difference of phase), was usually displaced from the original position an amount depending upon the pitch of the tone and upon the particular *O*.

There is an early paper by Thompson[53] in 1878 which indicates the importance of phase-differences as effecting localization, although Thompson also noted the effect of intensity. He noted the change in localization of the tone when the connections to a telephone receiver at one ear were reversed while the connections to a receiver at the other ear remained unchanged.

In recent literature there is a tendency to consider intensive differences as mediating a rough localization and differences of phase a fine localization. Thus Bowlker[54] believes that "in the case of the higher notes—perhaps in the case of all notes—the zone or arc in which the sound-image appears is settled by the relative intensity at the two ears; the actual position of the images within this zone being produced by phase-difference at the ears." Klemm[55] found experimentally that, for sounds of short duration, the in-

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[51]Rayleigh, *Phil. Mag.* (6 ser.), 1907, 13, 214-232.
[52]Stewart, *Phys. Rev.*, N. S., 1920, 15, 425-445.
[53]*Op. cit.*
[54]Bowlker, *op. cit.*, 327.
[55]Klemm, *Arch. f. d. ges. Psychol.*, 1918, 38, 88-91.

tensive differential threshold was significantly greater than the directional differential threshold for localization; that is to say, the image may move a distinctly noticeable distance in localization without the difference in intensity at the two ears becoming distinguishable.

The upholders of the binaural intensive ratio as the primary condition of localization cite the work of Matsumoto[16] in 1897. He used two telephone receivers, one at each side, simultaneously actuated by a 250 d. v. fork. When the relative intensities were altered the tone was variously localized about the head. These results seem to be inconsistent with the findings of the present paper. It must be remembered, however, that Matsumoto's tones must have been relatively impure, whereas ours were relatively pure in spite of the presence of the second partial in them. The addition of partials or noises to the fundamental alters conditions in an unpredictable manner. When phase is varied with an impure tone the various partials swing through different arcs to their respective angular limits (*cf.* p. 198 ff.); the relation of intensive change to the shift of the partials of a clang or the shift of different tones of different pitches is not known. Moreover, it may be true that Matsumoto did not always avoid varying the relative phase of his sources of sound, for he used an inductorium, the induction of which may have altered the electrical phase of the current. The greatest difference between the present experiment and Matsumoto's, however, lies in the lack of the introspective attitude on the part of Matsumoto's *O*'s. Our results have shown a certain amount of movement of the image about the median plane when the binaural ratio was altered, and they have shown that the tone (the 'second' image) 'reaches' the aural axis when the intensive difference has become great. Our introspections indicate, however, that the shift of the tone to the aural axis is discontinuous; it passes through a stage of equivocal localization where it is diffuse, scattered, or doubled. Matsumoto had no introspective check upon the nature of his *Os*' images; there is no certain way of telling whether the change for them was continuous or discrete.

In general then, it appears that a variation of the phase-relations of a tone at the two ears may lead to a continuous change in the localization of the tone, but that the variation of the intensive relations leads to a change that is continuous over a small region and discontinuous in its maximal change. Quantitative data, taken without introspective regard to the nature of the image of localization, may give the appearance of a continuous change of localization when the intensive relations are altered, because in the equivocal cases, where the discontinuity appears, an image that is double or scattered may be 'compromised' by being reported as localized at its geometrical center.

Conclusions

This study is a report upon the binaural localization of tones by four observers.

All these observers localized the tones in visual terms by placing a visual image that stands for the tone within a visual schema that represents the field of space. The analyses of the process of localization are given on pp. 186 ff., 198 ff.

The first part of the study shows that localization may be a function of the phase-relation of the tones at the two ears when the tone is conducted to the ears through closed tubes. The

[16] M. Matsumoto, Researches on Acoustic Space, *Yale Psychol. Lab. Studies*, 1897, 5, 1-75.

nature of the function is shown in Fig.. 1, and the results of this section are summarized on pp. 187-188.

The second part of the study exhibits localization as a function of phase-relation when the sources of sound are on either side of the head and the ears are open (no conducting tubes used). It appears in these series that the first and second partials of a tonal complex may be simultaneously localized and that each then follows the law of phase-difference independently of the other. The functions are shown in Figs. 2, 3, and 4, and the results are summarized on pp. 198-200.

The last part of this study shows the dependence of localization upon the intensive differences of the tones at the two ears (binaural ratio). It appears that with intensive variation the localization may move slightly, but usually remains in the region of the median plane of the head, except that with extreme intensive variation localization appears at either side of the head near the aural axis although it does not move there continuously. The shift of localization due to intensive change is thus discontinuous and not regular as it is for change of phase. It would seem that difference of phase is thus a more effective factor in determining localization than is the binaural ratio. These results are shown in Figs. 5 and 6 and in Tables III and IV, and the issue is discussed on pp. 209-211.

MACH'S "LECTURES ON PSYCHOPHYSICS"

By E. B. Titchener

In 1863 Ernst Mach, then *Privatdocent für medicinische Physik*, gave at Vienna a course of lectures on psychophysics.[1] We fortunately possess the reports of this course furnished by Mach to the *Oesterreichische Zeitschrift für Praktische Heilkunde* and there published in the same year. I believe that the lectures are practically unknown; and as they seem to me to be of more than merely historical interest I have made a brief summary of the contents of the reports,—a summary, be it remembered, of what is already part-summary and part-excerpt; the reader must expect nothing more than the barest outline. I have followed the divisions of the *Zeitschrift*, though I am not sure whether they represent separate lectures or available editorial space. Apart from the general headings, which I have ventured to supply, the rest of this paper is, then, a condensation of Mach's work.

The Science of Mind

§1. There is no special group of 'exact' sciences. Whether a science is or is not 'exact' depends solely on the stage of development to which it has attained. In fact, there is no reason why psychology (the science of the phenomena of the mental life) and psychophysics (the science of the interconnection of physiological and psychological phenomena) may not, if they follow the path they are already traveling, presently become in the fullest sense of the term 'exact' sciences.

[1]See my *Experimental Psychology*, II., ii., 1905, xlvi. The reports were reprinted in a little book, entitled *Vorträge über Psychophysik*, which I have never seen. For several years I sought it assiduously, but in vain; finally, I wrote to Mach himself about it. He replied that he knew of the existence of only one copy, which was in his own library; and with his usual kindliness—in such matters he was the soul of generosity—he offered, since I was seriously interested, to lend me the precious volume. I could not, of course, accept that risk; and as a reward of virtue I presently picked up the volume of the *Oester. Zts.* from which the present summary has been made (ix. Jahrgang, 146, 167, 202, 225, 242, 260, 277, 294, 316, 335, 352, 362). The volume, as it happens, contains also Mach's review of Helmholtz' *Tonempfindungen* (915, 930, 953).

xact' sciences? Mechanics, applied
ar as it is applied mechanics, physiol-
d physics, the science of man in so far
these disciplines rest upon exact laws.
hat is either because there is no law to
aw is too complex for immediate detection.
xperiments made, in accordance with the
varying the magnitudes involved, with the
shing a natural law, always yield discrepant re-
influences are at work, influences which we
the name of 'chance'. But chance itself is subject
uetelet has shown that, as regards man, not only
ase of population, etc., but also crimes, suicides,
wrongly addressed envelopes, etc., are subject to
law, that is to say, the moral and intellectual ele-
ur social life, the psychological processes, are no less
an the rest.

selves shall be mainly concerned with psychophysics, 's (as Wundt has recently shown) are of practical for medicine. Meantime we must briefly consider 's psychology, as the first attempt to deal with psychol- nomena from the mathematical point of view. The by no means ridiculous, since ideas, if not measurable at any rate vary in intensity and by that property mathematical treatment.

Herbart's Psychology

Herbart's psychology deals with artificially simplified just as mechanics does. Ideas once aroused are, indestructible; if driven from consciousness by other they persist as tendencies to ideation (cf. the law of matter). Disparate ideas are compatible; similar more or less antagonistic. Inhibition is mutual, and minimal; its amount in the given case is inversely al to the intensity of the ideas concerned (Wundt has how experimentally that the weaker of two ideas, even xtreme difference of intensity, is never wholly suppressed), and the clearness of the ideational fragments remaining after it has done its work varies roughly as the square of the intensity; *i. e.*, an idea twice as strong as another remains about four times as clear as that other, and so attracts the attention. A number of concurrent ideas may establish an equilibrium, with complete suppression of weaker ideas; mutual inhibition operates as if these weaker ideas did not exist. Hence we have but relatively few ideas present in consciousness at any one time, and hence we are not burdened and restricted by the we have forgotten. Equilibrium is not achieved in a

moment; the nearer it is approached, the slower is the advance towards it; indeed, it is never absolutely complete. In this way we account for the motility and variability of our internal states.

Ideas which concur in consciousness form permanent connections, and form them the more completely the freer the concurrent ideas are from inhibition. Here is the explanation of association and mediate reproduction. A series of ideas, whose earlier terms are increasingly inhibited as the series proceeds, allows only of progressively weaker connections between these earlier and the later terms; if the whole series has been inhibited, and presently a member returns to consciousness, the terms are reproduced in their original order. Herbart thus explains not only our remembrance of poetry but also the building up of our spatial perceptions.

Herbart's mathematical results come so near to the facts of experience that we must believe him to be on the right path. The good observer can, so to say, actually feel within him the struggle and mutual suppression of his ideas. But we turn now to experiments on sensation. If we find that sensations are measurable, we remove one of the principal objections to Herbart's psychology.

The Methods of Psychophysics

§4. The problem of psychophysics is to determine exact relations between stimulus and sensation by the way of observation and experiment. We have to measure both stimulus and sensation. The measurement of stimulus is simple; but sensation can be measured only by recourse to stimulus. It seems that we are involved in a circle.

Fechner, however, has shown us how to overcome this difficulty. We measure sensation by aid of the just noticeable difference of stimulus, which corresponds always with an identical increment of sensation, and thus furnishes us with a sensation-unit. We must, it is true, employ different sensation-units for the different classes of sensation, and cannot reduce them to such common terms as physics has found in mass, space and time; but there is ground for hope that a reduction may presently be effected.

The special methods of psychophysics seek to determine points on the path of a curve, *i. e.*, to ascertain for a selected number of stimulus-values those increments of stimulus which condition determinate increases of sensation. They are as follows.

Method of Just Noticeable Differences.—"For a series of stimulus-values x_1, x_2, x_3,............we determine the just noticeable differences Δx_1, Δx_2,............which make $x_1+\Delta x_1$ just distinguish-

able from x_1, $x_2+\Delta x_2$ just distinguishable from x_2, and so on, and which therefore in every case correspond with one and the same increment of sensation Δy. If it turns out, *e. g.*, that Δx_2 is twice as large as Δx_1, it necessarily follows that for the stimulus x_2 the rise $\Delta y / \Delta x$ is only half as large as for x_1. If we should find in general that the 2, 3, 4,............n-fold stimulus has the 2, 3, 4,............n-fold just noticeable difference, we could infer that $\Delta y / \Delta x = a/x$, *i. e.*, the increase would be inversely proportional to the stimulus-magnitude x. Mathematics would then draw the further inference that $y = a$ (log x/b), where a and b are constant magnitudes."

Method of Average Errors.—"We take a constant stimulus x and try again and again to make a variable stimulus x_1 equal to it; we determine the error of every trial, and from a large number of these errors calculate the average error. In comparing our sensations we are thus subject to error for the reason that a series of accidental circumstances influences our judgment. For example, we take the two sensations y and $y+\Delta y$ and therefore also the two stimuli x and $x+\Delta x$ for equal, and so make the error Δy in estimating sensation and the error Δx in estimating stimulus. The error Δy, by the laws of probability, cannot exceed a certain magnitude. The average error will also, in a long series of observations, keep within certain limits of magnitude. It is, however, clear that one and the same error Δy will have corresponding with it a larger error Δx, the more slowly Δy varies with Δx, *i. e.*, the weaker the rise of the sensation-curve. Hence the average error in our estimation of the stimulus x is inversely proportional to the rise of the sensation-curve for the stimulus x."

Method of Right and Wrong Stimuli.—This method "is a modification (or rather, inversion) of the method of average errors."

The Facts of Psychophysics

§5. We pass from the methods to the facts of psychophysics.

I. *Weber's Law*, so named by Fechner, declares that the just noticeable difference is proportional to the magnitude of stimulus. Fechner in particular has shown that, within certain limits, the law holds in the most various departments of sensation.

(1) *Intensity of Sensations of Light.*—Here belong Fechner's experiments with cloudlike figures painted on white paper; Volkmann's experiments with shadows; Masson's observations of grey-ringed white discs. All experiments show that not absolute but relative differences are important for us. A stim-

ulus must increase by a determinate aliquot part if the increase is to be remarked; and the same stimulus-difference is noticeable or unnoticeable according to the magnitude of the stimulus.

The law does not hold for very high and very low degrees of illumination. Helmholtz' experiments with rotating discs seem to show that it holds, in any case, only approximately. Fechner contends, however, that the external stimulus must always be increased, for purposes of calculation, by the amount of the intrinsic retinal light (determined by himself and Volkmann as equal to the light intensity of a black velvet surface illuminated by a stearin candle at a distance of 9 ft.). The results of observation are in this way brought into much closer agreement with the law.

(2) *Intensity of Sound.*—The experiments of Fechner and Volkmann confirm the law.

(3) Our sensations of *tonal pitch* and *interval* follow the law (Herbart, Drobisch); our sensitivity to *color* does not.

(4) Weber found the law to hold for our sensations of *resting and lifted weights*. Fechner's numerous and exact experiments with lifted weights give a good agreement with the law if the weight of the lifting arm is taken into account.

(5) Fechner believes, as against Weber, that we may have an uniform sensation of *warmth* or *cold*. It seems that the law applies to temperatures which differ but little from the mean (unsensed) temperature; it certainly does not apply to those that depart widely from the mean.

§6. (6) According to preliminary experiments of Fechner's the law holds for *mixed* sensations (colors).

(7) It holds also (Fechner, Volkmann, Appel) for *distances* and *lengths*, which are probably the resultants of a complicated psychological process. In the case of distance, *e. g.*, the facts of observation are satisfied if we regard the average error as made up of a constant error, identical for all distances and due to the division of the retina into a finite number of sensitive elements, and a variable error, proportional to the distance.

(8) Czermak's work on the sensation of *time* is preliminary only. Mach's experiments with pendulums (1860) prove that the law applies to this sensation.

II. A second fundamental fact is the *Law of the Limen*. Psychophysics distinguishes stimulus and differential limens, intensive and extensive limens.

III. Fechner's *Parallel Law* (that reduction of sensitivity has, at least in many cases, the same effect as reduction of stimulus) needs further investigation. It seems to hold for lifted weights, but not for sensations of light.

Fechner's Interpretations

§7. Weber's Law may be formulated as $\Delta y/\Delta x = a/x$, where a is a constant. By purely mathematical reasoning we derive from this expression Fechner's metric formula $y = a \log (x/b)$, where b is a second constant. Since $x=b$ means $y=0$, x must be $>b$ if y is to attain to a finite positive value; b then denotes the liminal value of x. The Law of the Limen would therefore be contained in Weber's Law if Weber's Law itself were unconditionally valid. As things are, the laws stand side by side, empirical and not contradictory.

The metric formula may be made definite if we take as unity the sensation which corresponds with some determinate stimulus-magnitude, and define a accordingly.

This constant a is a measure of absolute sensitivity; it is by no means to be confused with the liminal value b. At present, it is true, we cannot apply a measure of absolute sensitivity, since we cannot compare the magnitudes of the sensations which, in different states of sensitivity, correspond with the same stimuli.

Fechner's further arguments regarding the 'aggregate' sensation and its summation from single sensations are noteworthy and interesting; but neither his psychology nor his mathematics is free from objection.

§8. Fechner extends his metric formula, first, from differences of sensation to sensations of difference, which he thinks enter into the aggregate sensation, under certain conditions, as sensations of contrast; and secondly, in the instance of tones, from a single dimension—pitch or intensity—to the two dimensions taken together. Again, his arguments are open to objection. Mach (1862) applies the formula to our sensitivity for the change of position of straight lines; theory and experiment are here in good agreement.

Sight and Hearing

Our path now takes a new turn. Natural science begins always with problems that force themselves on the attention; involuntary enquiry precedes voluntary, as reflex precedes voluntary movement. Laplace's planetary theory arose from the insistent demand of the facts themselves. And, as in astronomy, so it is in psychophysics. Light-stimulus and sound-stimulus are both alike oscillatory in nature. Why, then, does tonal pitch obey Weber's Law, and color refuse to obey? Why does the tonal series show the periodicity of the octave, and the color-series lack periodicity? Why are tonal compounds analysable, and color-mixtures unanalysable?

These questions can, in large measure, be answered. We come presently to the special investigations (Young, Fechner,

Helmholtz). Here we must refer briefly to the laws of oscillatory movement under the influence of variable forces (Seebeck, Mach), laws which explain, among other things, the phenomenon of sympathetic vibration. Our interest lies in the theory of hearing; and we find in the tuned series of Corti's fibres an apparatus which, in terms of these laws, enables us to account for tonal analysis. The beats which arise when two near-lying tonal stimuli act on the same fibre, and which cause a disagreeable sensation, solve (or at any rate simplify) the problem of consonance and dissonance.

§9. Musical tones consist of fundamental and overtones, which latter determine timbre. If two or more musical tones are sounded together, we hear a dissonance or a consonance according as fundamentals, overtones or combinational tones do or do not engender beats. If we sound a tone with its first overtone only, and gradually raise the pitch of the tone to its octave, we experience a 'feeling of recurrence', of the periodicity of the octave-interval. Other intervals show analogous phenomena.

The theory of Corti's fibres was proposed, almost simultaneously, by Fechner and Helmholtz. How shall we test it? (*a*) Fechner suggested that, if single fibres are incapable of vibration, the ear will lack the sensations of the corresponding tones. Politzer confirms this suggestion. (*b*) Unilateral affection of fibres should bring it about that the two ears hear the same tone differently. Cases have been observed. (*c*) Any lessening of the capacity of the fibres for vibration should decrease our sensitivity to tonal differences. (*d*) Since the fibres are limited in number (about 3,000), our estimate of very small differences of pitch will perhaps prove to be subject to a special constant error (cf. what has been said of the estimation of spatial distances).

We have seen how the sensations of color differ from those of tone. We add (*a*) that the mixture of two or more spectral colors gives rise in all instances to a color which can be represented by white and an intermixture of a single spectral color; and (*b*) that any mixed color whatever can be produced by the mixture of three properly chosen spectral colors. Brewster thought, accordingly, that there are only three objective colors; but Helmholtz has proved the continuity of the spectral series. Helmholtz and Fechner (who, again, published almost simultaneously) therefore suggest, following Young, that there are three fundamental subjective colors, red, green and violet. But Helmholtz believes (with Young) that each one of these colors has its own nerve-fibre, while Fechner assumes three different kinds of process within the single nerve-fibre. The point at issue will be decided the one way or the other according as the retina of

the partially color-blind (who lack a green-sensation) turns out to be defective in anatomical or in chemical elements. Or we might examine the color-blind periphery of the normal retina.

Since every objectively simple color is subjectively compound, no one has ever seen a simple color. If we look at a light for some time through a red glass, and suddenly exchange the red for a green, we see a green of quite unusual purity.

In summary: "the eye complicates the objectively simple, the ear analyses the objectively complex. On the other hand there is also a real resemblance. We might say that the eye is a mosaic of light-ears, with the number of Corti's fibres reduced in every ear to three.—These differences seem to account for at least the greater part of the differences in the behavior of our sensations of light and sound."

§10. Helmholtz' extension of the doctrine of specific energies, which follows on his theories of color and tone, is to be regarded with caution. Helmholtz thinks that not only sensations but also qualities of sensation have their special nerves; since, however, we find in the nerves only electrical currents, these nerves must be like telegraph wires, and simply transmit certain signals that correspond with the sensations. We know, however, too little of nervous processes. What we know comes, it is true, by way of electrical experiments; but these experiments, delicate as they are, are also too crude; effects that seem to be physiologically the same need by no means be physically identical.

The Problem of Perception

We have already objected to Fechner's view of the aggregate sensation as a mere sum of simple sensations. Wundt has recently shown (1862) that between sensation and perception there lies in fact a series of psychological processes, which he calls 'unconscious inference.' Helmholtz had previously come to practically the same conclusion. Wundt, *e. g.*, has proved the dependence of the perception of distance upon accommodation and convergence; he has proved further that there are no 'identical' retinal points. All Wundt's experiments on visual space-perception indicate that the sensations of the two retinas come separately to consciousness, and only there are connected, by a sort of inference, to a stereoscopic perception; and what holds of stereoscopic vision holds also of mirroring and lustre.

What now is the function of these unconscious inferences? Kant regarded space and time as forms of perception given a priori: but why then should some sensations (sight, touch) fall into the spatial schema, and others not? Weber makes the brain repeat the peripheral arrangement of nerve-endings: but here is not even an explanation of tonal analysis, to say nothing of

space-perception; and even if the theory be taken to account for our separate hearing of partial tones, it would still be inadequate to space, since the two modes of perception are not identical. Herbart explains space-perception as a case of serial reproduction: but on this hypothesis, again, all the senses ought to be spatially competent.

§11. Lotze, in his doctrine of local signs (which may be either a system of concomitant sensations or a system of movements, or rather of impulses to movement), attempts a combination of the anatomical and the psychological views. Wundt's theory is an extension of Lotze's. The part which Wundt ascribes to muscular feeling (against Schiff) is rendered highly probable by the results of his experiments on the estimation of distance. His theory takes account not only of visual space-perception but also of the tactual space-perception of the blind and the seeing. It is supported by observations of pathological phenomena (paralysis of the ocular muscles). Wundt has further observed that the 'sensory circle' very quickly changes under the influence of therapeutic treatment; and narcotics affect the retina as well as the skin. It may be that a diminution of sensitivity means a diminution of the intensity of movement-impulses; in that event we need not, with Fechner, modify Weber's theory of the sensory circles.

Mind and Body

§12. The materialistic point of view is untenable; it simply puts new prejudice and untruth in the place of old. Fechner carried out all his psychophysical investigations without setting up any hypothesis of the nature of the psychical, and without making any assumption regarding the processes that lie between stimulus and sensation. He suggests, however, in his *Psychophysik*, that physical and psychical may be the same thing looked at from different points of view (cf. the concave and convex aspects of a circle). We may arrive at a like hypothesis by the road of physics. For the facts of physics indicate that we have to do with a finite number of entities and forces, with what are called 'atoms.' Try now as we may, we can frame no rational idea of an 'external aspect' of these atoms; we are forced back upon an 'internal aspect,' in some sort analogous to our own 'mind'. And indeed, how should we ever get to 'mind' at all if the germ of mind were not present in the atom?

Fechner also raises the question of the 'seat' of mind in the organism, and finds it in the whole nervous system. There are difficulties in this view, but they are not insuperable. Fechner further considers the lower animals and the plants, and refuses (rightly) to set any limit for the beginnings of mind. We reached the same result in our consideration of the atom.

In summary, these lectures have tried to teach two lessons: "(1) that physics, physiology and psychology are inevitably bound up with one another, so that each one finds its salvation only in the companionship of the others, and each one may be regarded as handmaid of the others; and (2) that we are not bound to despair of exact investigation when we pass beyond the borders of the palpable."

Herbart and Wundt

Postscript.—Wundt has earned our gratitude by reducing the many riddles of the psychology of perception to a single riddle. But he is unfair to Herbart. (*a*) If the principle had always held good that science cannot be developed out of metaphysical hypotheses, we should never have had a mechanics, to say nothing of a psychology. (*b*) Those who know the Herbartian psychology assure us that it contains much more than could be discovered by mere attentive observation. (*c*) Wundt's criticisms in the *Beiträge* confuse intensity and clearness of ideas. Herbartians themselves do not always escape this error. (*d*) Wundt's experimental disproof of Herbart's two-idea minimum fails: it is as impossible to experiment with two ideas as with two atoms; and Wundt confuses idea with sensation. If Wundt's explanation of his complication-experiment were correct, we should have a mosaic of ideas, but nothing like connected thought.—Herbart has in fact made a very noteworthy contribution to scientific theory. It is no reproof to a young science to say that it proceeds on too simple assumptions. As a rule, Wundt's enquiries begin where Herbart leaves off.

THE DEVELOPMENT OF MEANING

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Introduction. The following introspections and interpretations thereof are here presented as a constructive criticism of the recent investigations of Meaning by Moore and McDonough,[1] and as a forerunner of a detailed theoretical discussion of the problem of Meaning.

In an investigation of Choosing[2] it was found that in the act of giving a detailed introspection the observers not only found themselves giving a description of immediate content, so-called, but also found themselves interpreting this content even as it took place. The more detailed the introspection, the sooner after this content appeared did the reagent interpret it. In other instances the reagent did not so interpret his experiences until they had passed and gone.

These facts led to the conclusion that many of our so-called unanalyzable mental processes such as Attitudes, Thoughts, Consciousness of the Self and the like are interpretations, which the observers have failed to analyze owing to lack of training or ability. These facts also led to the conclusion that such "interpretative periods," as these tendencies were called, are legitimate features of any introspection, providing they are properly analyzed and recognized as such.

These "interpretative periods" are no more and no less than the development of meaning. They are complex meanings in that they are definitely analyzable into sensory and imaginal components and involve a definitely describable behavior of attention.[3]

In the experience of the writer it has very frequently turned out that the development of meaning, itself, may assume various forms under the guise of "feelings of meaning," "awareness of meaning," "consciousness of familiar meaning," "recognition

[1]McDonough, A. R. The development of meaning. *Psychol. Monog.*, Vol. 27, 1919, 443-515. Moore, T. V. Image and meaning in memory and perception. *Psychol. Monog.*, Vol. 27, 1919, 69-296.

[2]Wheeler, R. H. An experimental investigation of the process of choosing, *Univ. of Ore. Publ.*, Vol. 1, No. 2, 1920, 59 pp.

[3]See Wheeler, *op. cit.*, 33ff. 51 f.

of meaning" and the like, according to the observer's predisposition to label such experiences in one fashion or another. But just as in the case of the attitudes—acceptance, rejection, surprise, etc.—and consciousness of the self, "awareness of meaning" and "feelings of meaning" are interpretative periods. They are in themselves analyzable and constitute a stage in the development of meaning or in the recognition of meaning when the reagent is assuming an introspective attitude. When the reagent is not assuming an introspective attitude any attempt to describe them fails, for the contents are so numerous and their duration so fleeting that one's attention is entirely unable to cope with the complexity of the situation.

The following introspective data are typical of a vast amount of material of all sorts and descriptions which might be offered in demonstrating that development of meaning need not escape the observer even in its minor details. The introspections are the author's, obtained in the laboratory, with the aid of an expert assistant.

Introspective data. As far as possible, "interpretations" are set off from "descriptions of content" by means of parentheses.

1. *Instructions:* "You will be given a word to which I want you to react as soon as you are conscious of the meaning. Ready, now."

KNIFE. "At the outset I was focally conscious of the word as spoken by the experimenter; attention centered itself upon the long 'i' sound of the word, momentarily, and then shifted to the 'f' quality. These processes had hardly developed when I found myself becoming tense about the vocal organs and in my right arm. Together with these motor reactions there appeared visual imagery of a knife with two blades protruding at right angles to the body of the knife. One blade was smaller than the other but no other details except the steel color and the shape of the blades stood out in this imagery. Then the handle of the knife clarified somewhat and I saw in this imagery the small wrinkles and the varying shades of brown and black which characterize such a handle. This imagery did not develop very far, for at once, breaking into consciousness, was the 'idea', 'cutting instrument'. This idea consisted first of a slight shift of my line of regard from the visualized knife upward and to the right, where I visualized what might have developed into the complete image of a paring knife; but in this latter imagery there appeared only the broad thin blade with the cutting edge upward and shining as if it had recently been sharpened; along the edge were tiny scratches as if made by a sharpening instrument; and at one end of the blade there showed about one-quarter inch of a reddish-brown handle. (In describing the details of this imagery the very use of language makes it seem as if meaning were present when it had not yet developed. I was not aware of the fact, at the time, that I was imaging a paring knife; nor was I aware of the fact as such that the paring knife had recently been sharpened. I have merely been describing the features which stood out in the imagery.) Meanwhile, the motor tenseness which had developed at the outset persisted and my attention was again claimed by it; it was increasing in intensity and was spreading to take in the muscles of my chest and shoulders. (Up to this time the kinaesthesis had constituted the beginnings of a recognition-consciousness, or a "feeling of mean-

ing," or the beginnings of a consciousness that the word 'knife' was familiar.) Just as this motor reaction was spreading I had in vague, fleeting terms the vocal-motor image of 'cut'; but before further verbal imagery came my attention was again claimed by the bodily kinaesthesis. (Here the meaning 'knife-as-a-cutting-instrument' had begun to develop. It seems as if I should then for the first time have been able to say that the meaning of the stimulus-word had become conscious.) The verbal 'cut' then elaborated to the vocal-motor 'cutting instrument.' Here the motor reaction increased still more in its scope. (I was reacting to the 'meaning'; but here the reaction was merely an intensification and prolonging of the kinaesthesis which had already constituted the core of the meaning itself.) I found myself attending to the experimental surroundings, momentarily, as if the task had been completed; relaxation tended to set in, but this was inhibited by the development of further meaning. The 'idea' occurred to me that 'knife' might mean an 'instrument used in eating.' This is an interpretation which followed a flashy visual image of a silver eating-knife poised as if in someone's hand but with no hand visible; it was located in front of a very diffuse and vague white background which I later interpreted, vocal-motor fashion, to be a table-cloth. The class-meaning 'instrument' was not present to consciousness as such except in the vocal-motor tendency to define the knife; this latter tendency appeared at once upon the appearance of the visual imagery of the eating-knife, and gave way at once to an awareness of the instructions and to the *Aufgabe*-consciousness to begin to introspect. At no time during the experience was there any meaning present to consciousness as such other than in imaginal or in kinaesthetic terms."

2. *Instructions:* "You will be presented, visually, a word to which I want you to react after you have mentally defined its meaning. Ready, now."

PAIN. "I reacted at once to the meaning 'sensation, pain.' As my line of regard fell upon the printed word I perceived it at once as a whole, and before I became conscious of the fact that I had thus perceived the word as a whole there developed a muscular jerk about the left shoulder and a tenseness in the throat. (This 'jerk' constituted a feeling of meaning.) Momentarily the quality of tension stood out focally in consciousness, while at the same time I was still, non-focally, perceiving the stimulus-word. (This broadening of the span of attention to take in the stimulus-word and the motor reaction made it possible for me to say that it was the stimulus-word which seemed to possess the meaning. The kinaesthesis was the content of the meaning-consciousness itself.) Then, as this 'jerk' persisted, I had a tactual image of pain so brief and fleeting that it was not at the time localized; it was merely a pain quality, punctiform and sharp; but almost as it came, it disappeared. Momentarily, and after the pain-image had vanished, I tended to visualize the outer surface of my left shoulder as if I were now trying to localize the pain. Along with this I was vaguely conscious, in terms of visual imagery, of my bodily position and of my line of regard as it was extended in the direction of my shoulder. All of this happened before the muscular 'jerk' in the shoulder disappeared. (This latter tendency to visualize the entire situation constituted an interpretative period, an instant in which I was conscious of the fact that meaning had developed. In other words the kinaesthesis itself in the presence of the pain-image constituted the meaning 'sensation of pain,' while the visualization of this kinaesthesis constituted my consciousness of the meaningfulness of the entire experience. When the kinaesthetic 'jerk' became supplemented by visual imagery and the broadening of the span of attention, the interpretative period had developed. There was positively no meaning present over and above what I have described.) I then found myself relaxing. But before this relaxation had completed itself I thought

in another meaning. The stimulus-word then meant 'window-pane'. This developed as follows: I found my line of regard shifting upward and to the right from the visually perceived word, 'pain', and here there tended to develop vague visual imagery of a window, the only features of which that stood out were a blue-grey, smooth surface, translucent but not transparent, as if it might have been ground glass; but I was not conscious of the fact at the time that it might have been ground glass. This consciousness comes only in the telling of the experience. This glass, in my imagery, was surrounded by sash, on two sides, about an inch wide; the sash was dark, with a very faint tinge of brown. My visual image did not take in the left or bottom edge of the pane. My visual attention was centered upon the blue-grey quality of the pane and upon its smooth surface. At that time I was not aware of any verbal tendency to say 'pane,' but immediately after this visual imagery had flashed into consciousness I found myself saying 'window-pane,' and as I did so my visual attention increased in scope to take in the distance between the projected pane and my head. This latter experience constituted a recognition of the meaning of the experience—an interpretative period. The development of the first meaning in connection with the stimulus-word took place so rapidly that it was all over before my line of regard had swept across the word from left to right. The final meaning developed almost as quickly, and came during the last stages of my motor reaction. The background of kinaesthesis became the vague content of my final interpretation period, and it was toward this kinaesthesis that my attention was spreading during the interpretative period."

3. Same instructions as before.

SCULPTOR. "I had hardly perceived the word, visually, before I found myself reacting to the meaning. My line of regard fell at the outset upon the first four letters of the word, and before it had shifted far enough to the right to see the remaining letters in focal vision I filled out the word in visual imagery of the remaining letters, together with vocal-motor imagery of the entire word. The latter was exceedingly clear-cut and vivid, especially the imagery of the throat and lip positions in saying the 'tor' portion of the word. Together with this emphasis upon the 'tor' there appeared visual imagery of the 'o'. Along with all of this imagery there suddenly developed tensions about the chest, shoulders and in my right arm. I was already beginning to react. (This kinaesthesis constituted a "feeling of the meaning"—a recognition of the word as a familiar one; but as yet there was nothing in consciousness that involved the concrete meaning of the term. At this juncture I was not aware of the meaning as such. So far the processes might be summed up in the phrase 'I know the meaning of the word.' Neither did this response itself mean familiarity as such; it meant familiarity in virtue of an immediate tendency to review my experiences thus far and to label them. This was done in terms of a tendency for the span of attention to widen and in a tendency to visualize the kinaesthesis and the word itself at the same time.) Then my line of regard shifted in imaginal fashion to a region directly above the printed word; there I visualized a man, very indistinctly and only in part. Here I labelled [illegible] the time; I am describing only the details of the imagery. [illegible] half of a square jaw, a sunken cheek, very dark skin, wrink- [illegible] fashion as to indicate that it was very coarse and rough; above [illegible] shaggy, frizzled hair; below was a glimpse of one side of a [illegible] coat and of one trouser-leg, dirty, unkempt and [illegible]. The coat and trouser-leg were black. Off to the [illegible] merged into indefinite blackness and from hence into [illegible] this imagery merged into an undifferentiated area of [illegible] that might have developed into the details of a room.

I then noted in this imagery that the man's right arm was outstretched; I found myself tending to look in the direction of this outstretched arm, but did not actually do so; I was merely aware of incipient eye and neck tensions of turning in that direction. During this time my motor reaction had persisted and had become more intense, constituting a reaction to the entire situation—the stimulus word and the imagery. (Again I was reacting to familiarity and to but the beginning stage in the development of definitized meaning. Up to this time the meaning was 'sculptor as an individual'; and the more fully developed meaning came only after I found myself reacting.) I was then conscious of a tendency for my line of regard to shift suddenly and vigorously to the right of the stimulus-word; there I found developing a visual image of something tall and brown; it did not become definite enough to describe in detail; it had three 'lobes' or sections as if it might have been the beginning of a visualized group of statues of three persons in bronze. This vague, fleeting and undifferentiated visual image rested upon a vague, dark 'something' below, and was projected in front of a dark green background, which latter had no definite limits in any direction. All of this latter imagery was accompanied by a sudden tendency for tensions to develop in my chest, arm and throat, and this tendency was distinctly perceptible over and above the antecedent movements which had to do with reacting. I then found myself visualizing the 'tue' part of the word 'statue,' and was aware of the word 'statue' tapering off in consciousness; I had failed to detect this latter imagery as it had entered consciousness. (All of this latter imagery, together with the motor response, constituted a recognition of the meaning, 'statue,' and was an interpretation period.) Then I had, rushing into consciousness, visual imagery of the head and neck of a horse together with the anterior portions of his shoulders; I was not aware at the time that it was a horse or that it was a marble horse. Brightness, form, shadows, the curve of the neck, jaw, the bony protrusion above the eyes, the forelock, were all featured in the imagery, but the whole experience lasted for only the smallest fraction of a second. (This imagery was the first stage in the development of further meaning—sculpture.) The last feature of the visualized horse to stand out in consciousness was the wrinkled musculature along the anterior-lateral portions of the near shoulder. As my line of regard was leaving this image and as the image was becoming indistinct, I again found vocal-motor imagery tapering off in consciousness. This time it was the vocal-motor 'sculptor'. Again I had failed to note the image as it had appeared. As this latter awareness of verbal imagery developed, part of it spread to a tendency to visualize my throat and the space between my throat and the vanishing image of the horse. (This latter experience constituted an interpretative period. I had now mentally defined the word 'sculptor'.) During the development of the two meanings, 'statue' and 'sculpture', there were two distinct motor 'sets'; these 'sets' differed slightly. The first involved slight eye-tension of holding my line of regard upward; the second involved eye-strain directed toward the right; the first was not as intense as the second; the first was confined more largely to the throat, chest and eyes, while the second involved incipient movement of my right shoulder as well. Both 'sets' constituted a background for the developing visual imagery and seemed essential to the development of meaning. It was to these kinaestheses that I invariably found myself referring the meaning itself. At no time during the entire process did I have any affective toning."

4. Introspection on the development of an illusion and its subsequent change in meaning: experienced as the reagent was about to enter the Psychology Building, and taken down at once upon reaching the Laboratory, less than five minutes later. In the meanwhile notes had been scribbled on paper.

"As I was walking toward the laboratory building I was very dimly conscious of my surroundings except now and then of a momentary visual perception of the walk in front of me. Passing focally through consciousness were trains of verbal images such as 'are the corrections too numerous', 'will the printer object', and the like. Together with these verbal processes there appeared fleeting visual images of Mr. H., the chief of the printing establishment, and visual images of presses, cases of type, etc. I saw Mr. H. in his familiar dark grey suit; he was standing opposite at a table and was slightly stooped over; his head was turned downward and slightly to the left; I did not visualize his face distinctly; I did not visualize the whole of the table, but saw its dark brown color, the thickness of the top, and a mass of papers scattered about. None of these features became clear during the brief existence of the imagery. (I seemed to be in the printing room as if I were talking to Mr. H. about the manuscript which I was actually holding in my hand, and saw these details as I should have done had I been at the table opposite him.) Then there appeared a mass of visual imagery of different pages of the manuscript. These were but fleeting glimpses particularly of such pages as were considerably marked with corrections. I then found my attention centered upon several words which began with 'w' and which I had retouched with ink. While my line of regard was thus skipping about from 'w' to 'w' and had reached midway down a visualized page and a little to the right of the center, I had the very fleeting and syncopated vocal-motor-auditory: 'Is there someone behind me?' in rising inflection. Up to this time the tactual-kinaesthetic sensations from walking had remained in the dim background of consciousness, as well as a kinaesthesis of frowning, and tensions from the bowed condition of my neck as I walked along in deep thought. At this juncture these kinaesthetic processes began to approach a higher degree of distinctness; then there suddenly developed the motor processes of turning my head, shoulders and upper trunk to look behind me. At this time there was a change in the 'set' of my facial muscles, but this was so diffuse that I can not describe it in detail. There was in this change a relaxation of forehead-muscles from the frowning condition of the moment before. Then rushing into consciousness came visual perceptions of my surroundings—the walk, the green lawn on both sides of the walk, near and distant trees, etc. As my line of regard swept over the distance between me and the trees and adjacent buildings I perceived more and more of the sky in the background, although in indirect vision. This entire change of scene took place very rapidly while I was turning round, but nevertheless the successive focussings of attention upon the near and then upon the farther objects were easily detected. By the time my line of regard was focussed some distance down the walk, and with my eyes centered upon the walk itself, I 'saw' in the sky, above the trees and out over the town several blocks distant, a huge electric sign, in indirect vision. Here my imaginal line of regard was centered upon the first letter of the upper left-hand word, which was clearly a 'W'. The remaining letters of the word and the other words as well were hazy and indistinguishable. But my imagery included the entire sign which had three rows of words, one row above another. It was a typical large electric sign in that the letters were of uniform size, large and blocked; the letters were black, but around them and behind there were faint suggestions of yellow, the yellow forming the sides of the block letters. Surrounding the entire sign was the blue-grey background of the sky. At the instant this imagery appeared and while my line of regard was still focussed upon the 'w' there developed a sudden kinaesthesis of surprise, consisting of tendencies for the musculature of my shoulders and neck to become fixed in a rigid position—a marked change from the gradual movement of the moment before, of tendencies to hold my breath and to droop my lower jaw, of a sudden circulatory change which appeared as a tightness or pressure in the left side of my chest, and of a wave of pressure welling

upward toward my throat. Along with this attitude of surprise there appeared syncopated verbal imagery of "Westinghouse Elec—." I cannot remember that the words were completed. Then my consciousness became focally visual again, and I found myself tending to visualize the words 'Westinghouse Electric Company,' but this imagery was very sketchy and fleeting and only a few letters here and there stood out. This imagery was localized below and to the right of the visual image of the sign and much nearer me. During the appearance of this latter verbal and visual imagery there was a decided change in the muscular 'set' of the moment before; tensions from the attitude of surprise became much less intense; momentarily I was aware that the sign 'meant' Westinghouse Electric Company. This awareness of meaning consisted, first, of a non-focal relaxing of tensions particularly about the neck and shoulders with visualizations of these regions; secondly, of pleasantness which I am totally unable to differentiate from a momentary relaxed condition of muscles about the mouth—a kinaesthetic consciousness; this region was also visualized as I became aware of the kinaesthesis; and, thirdly, of a very diffused bodily set which might be described as 'acceptance' but which did not mean acceptance to me at the time. This attitude consisted of a more general awareness in kinaesthetic terms of my whole trunk, my head and my neck. This was a diffused, relaxed condition, hardly more vivid in one region than another, but distinctly involving a wider region than at first and a decided change from the general muscular 'set' of the moment before. All of this was visualized and was accompanied by a 'set' in the throat as if to make some remark, with the vocal-motor 'I'. (This latter and very complex motor 'set' developed at once upon the appearance of the verbal and visual imagery described above. It constituted an interpretative period—a recognition of the meaning as such.)

No sooner had all this happened than I found myself turning back to pursue my way to the laboratory. The kinaesthesis of turning was hardly perceptible. I had no sooner turned than I again found myself looking back, this time directly toward the region where I had just 'seen' the sign. The transition of objects across the field of vision was again noticed, but this time non-focally. Consciousness was now occupied with the kinaesthesis of turning and with a wonder as to what the experience was all about. This wonder consisted of persisting throat tensions, in the suddenness with which I turned about the second time, and of a questioning consciousness, which latter was characterized by a focalized kinaesthetic strain in the neck as I turned my head around, upward and slightly sidewise along with the movements of turning my shoulders and trunk. There was also faint auditory imagery of something, too indefinite to describe. It was in terms of my own voice and sounded like 'Uhm'. Also there lingered in the background of consciousness the tightness of circulation in and about my chest and neck. As soon as my line of regard was directed into the sky toward the place where the sign had appeared, there developed very suddenly and with great focality a visual perception of what I had previously interpreted to be an electric sign—clouds of smoke. Here the meaning 'smoke from a factory' developed with a degree of vividness which was evidently rendered all the more focal in contrast to the meaning 'electric sign' of the moment before. The first feature of the visual perception to stand out focally was its brightness; attention was centered upon the brightness-quality of the smoke as if in contrast to the blackness of the letters of the moment before. (This statement about contrast is but an interpretation based upon the suddenness with which the brightness-feature of the smoke developed into consciousness.) This behavior of attention was only momentary, for the yellowish tinge of the smoke then stood out focally, accompanied by a non-focal verbal image of the word 'yellow' and a concomitant tendency to visualize a large yellow 'Y' very dimly localized upon the smoke itself as a background. This tendency was so fleeting as to

be almost overlooked. The next features of the smoke to stand out were its motion and its rolling cloud-like formation as it floated across the sky, spreading and becoming thinner as it did so. These details then gave way to incipient tendencies to follow the smoke down and to the right toward its origin, but this motor tendency, together with its shift in line of regard, had but just commenced when I found myself anticipating the source of the smoke by means of visual imagery of a black iron chimney and a red, flat-roofed building. With this imagery there appeared incipient verbal imagery of the word 'gas', and then 'factory'. Then at once a general bodily 'set', much like that described above, developed. (At this time I was not conscious of a difference in meaning nor was I conscious of a similarity in the two 'sets'. These facts are after-thoughts. The confusion comes in being forced to short-cut the introspection by drawing similarities and differences as one introspects. After the experience was over I became conscious of the differences of the two meanings and of the similarities of the two 'sets'.) This second motor 'set' consisted of a very general, wide-spread, intense 'jerk' or tenseness which began in my throat, spread to my face, and then down into my chest and about my shoulders. (I think that this is merely the order in which my attention took in these kinaestheses; it is a sort of introspective analysis which was going on at the time; for just previous to this behavior of attention the strains had developed but were so general and diffused that none of them was localizable nor were any of them definitely describable. They became describable after my behavior of attention toward them seemed to result in their definitization and localization.) Again these tensions seemed at first to be nothing more than a changed condition from the kinaesthesis of turning around. At this time the musculature was not visualized and there developed no attitude of acceptance and no interpretative period; I found myself tending to label the experience immediately afterward, verbal fashion, as the development of meaning. No intervening processes took place other than a tapering off of the kinaestheses and visual imagery from focal attention. This interpretation was a visualization of the region of my throat, together with a tendency to innervate the word 'meaning' and a still further change in the general motor background. This latter change consisted of localized relaxations about the mouth and eyes, while attention took in non-focally a general awareness of the 'resident' bodily kinaesthesis elsewhere, and also a kinaesthetic-static image of lifting myself upward. This was accompanied by dim visual imagery of my body from the trunk up. Much of this imagery and many of the sensory experiences were dim and fleeting. The latter group of experiences, constituting a tendency to label the entire last half of my reaction as the development of meaning, ended the entire affair. (Here the interpretative period was delayed; but we notice the same tendency for attention to broaden its scope and to take in kinaestheses *plus* visualizations of musculature.) Nowhere in the entire experience was there any hint whatever of imageless or sensationless meaning. On the contrary, I was distinctly conscious of the fact, immediately afterward, that images and sensory processes constituted the meaning."

Discussion of data. The reliability of such introspections as are given above has been checked repeatedly in the laboratory by means of giving two consecutive introspective accounts of the same process. In no instance have elaborations or important omissions been found. Constant experience over a number of years in giving introspective descriptions of complex experiences has convinced the writer that, elaborate as they are, the descriptions are faithful to the facts.

It is obvious that as far as this reagent is concerned there are no unique meanings nor are there any 'superstructures' of meaning unless one wishes to regard interpretative periods as 'superstructures.' But these 'superstructures' themselves are analyzable and are invariably sensory or imaginal in their content. Bodily kinaesthesès added to vocal-motor and visual processes change implicit into explicit meanings. Meanings seem never to be fully developed until the original contents of consciousness which function as the 'process-aspects' of meaning lead to an interpretative period. Without these periods meanings are implicit; they exist only in the behavior and context of original contents. With these interpretative periods the meanings become explicit; they become data of consciousness; whereas before the meanings were only data in consciousness. By means of these interpretative periods the reagent becomes conscious of meanings.

The above introspections are full of 'thoughts,' 'feelings of meaning,' and 'conscious attitudes,' yet none of them reveals the slightest evidence of meaning in the absence of image or sensation.

It is evident that, before an introspection upon meaning is complete, at least three stages must have been analyzed. (1) The reagent must describe what is known as 'original content.' This includes a statement of the imagery and sensation components present in consciousness. Such a procedure is possible only when 'immediate content' is reviewed by assuming an introspective attitude. In other words these original contents must lead to further contents before the former can be described, or before they can become data *of* consciousness. (2) Insofar as meaning is involved in original content it is implicit. By means of an interpretative period the reagent becomes conscious of the meaning. That is, the meaning becomes explicit. The introspector must analyse this interpretative period, else he will be projecting the consciousness of meaning back into the original contents where the meaning itself was not conscious at all but merely implied in the context and behavior of contents. This interpretative period, in this reagent's case, invariably includes a peculiar behavior of attention in which verbal and visual imagery are tapering off and in which kinaesthesis comes to the foreground and is visualized. (3) This second stage must lead to a third before the reagent can become conscious that the consciousness of meaning is an interpretation. This takes the form of a secondary interpretative period or the form of translating the experience of meaning into the language of introspection.

Where Stage 2 does not develop, the reagent is not conscious of the meaning, but in virtue of Stage 3 he is conscious that

the experience was meaningful and refers the meaning to kinaesthesis.

In Stage 1 appears a bodily 'set' along with verbal, visual, or auditory imagery. Imaginal content functions as the stimulus to which the bodily 'set' seems to be a response. Both groups of factors are essential for meaning. Stage 2 appears in terms of this lingering bodily 'set' while the stimulus-imagery is tapering off, and this 'set' is visualized. Attention here broadens to take in the space between the localized stimulus-imagery and the localized kinaesthesis. Both of these groups of factors are essential to Stage 2. Stage 3 includes this same kinaesthetic set, still visualized, but tapering off in consciousness, as one factor, and a verbal tendency to label the experience as meaningful as the second factor.

In each stage, therefore, two sets of factors are essential—a stimulus function and a response function.

Conclusion. The development of meaning involves three stages: (1) the original or 'given' process; (2) a subsequent process which interprets the first; (3) a third process which constitutes a final interpretation. The first stage is the original content of consciousness; the second stage is necessary before this original content can become a datum *of* consciousness; the third stage is essential before the meaning of this datum can become a datum of consciousness.

In other words there are (1) shifting or developing sensory and imaginal contents, and these must shift or develop before their antecedents have finally vanished from consciousness. Along with this development of sensory and imaginal contents there arises a motor 'set' or attitude. An image or a group of images in a context never 'meant' anything in the absence of this motor 'set' or of verbal imagery. (2) These data develop by means of a peculiar broadening of the span of attention to the stage of consciousness of meaning. Here attitudes become conscious attitudes. (3) The motor 'set' undergoes certain changes in emphasis and is supplemented by verbal imagery and further visualization of musculature. This constitutes an awareness that the meaning has been or is now being recognized.

One stage develops suddenly and with no line of demarcation from its antecedent. Stage 2 develops before the contents of Stage 1 have vanished and is a behavior of attention by which we 'know' what these contents are and by which original data *in* consciousness become data *of* consciousness. Stage 3 is a behavior of attention by means of which this knowledge becomes explicit rather than implicit, *i. e.*, by which a recognition is recognized.

Stage I consists of process-aspects of the so-called 'given' mental contents and meaning is absent until kinaesthesis appears. In every stage kinaesthesis is the core of the phenomenon which we call meaning. In every stage there is a shift in the emphasis which attention puts upon the contents themselves. It is quite evident, therefore, that kinaesthesis is vital to the development of meaning. In the writer's case, at least, meaning has not been described until kinaesthesis is taken into account. It is quite probable that pure meanings, so-called, are in reality masses of diffuse muscular sensations which the reagent has not succeeded in recognizing and in describing.

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productivity and his tendencies toward originality. If his creative ability is expressed in many of these tests, the methods of scoring have failed to take it into consideration. It is evident that we need tests designed to give us more direct and dependable information upon this essential element of progress—creative imagination.

II. How Creative Imagination May be Detected

Creative ability is marked by the initiative which one evidences by his power to break away from the usual sequence of thought into an altogether different thought. Progress is made by the realization of limitations. When these limitations are recognized, then a new approach is sought for an explanation of the problem confronting us—or should be sought.

Frequency of spontaneity in thought is the true measure of a person's creative capacities. A person might be pregnant with new combinations of ideas, and yet they would not have any special significance as being useful, artistic or unique. We are looking for deviation, not necessarily the utility of the creation or the beauty of a design. Creative ability is evidenced in one's tendencies to abandon old unfruitful paths for others. A searching type of mind, a combing mind, a synthetic mind is what we are looking for.

The number of figures drawn within 15 min. is no index of a person's creative imagination. This gives merely his speed in production. It has no quantitative or qualitative significance. The number of departures from following a previous figure in its form or utility, however, marks that quality of mind that is original and searching.

Initiative of deviation is for our purpose, then, accepted as the measure of a man's creative ability. This deviation may be either evolutionary or spontaneous. It may involve either mechanical production or inspired production. People vary as to initiative. The nervous individual, possessing aesthetic tastes and sensitive feelings, may possess more initiative or liberate more energy than the indifferent calm thinker. But initiative towards one's task is certainly no mark or evidence of constructive ability and originality. It is true that this may be a valuable asset to a creative mind, however, because of the facility which it engenders.

Now, what motivates these deviations? Ambition, that intangible incentive to action which looks forward to satisfaction, is absolutely necessary. Hedonistic motives no doubt exercise the predominant developmental urge.

All creations, such as novels, inventions, poems, etc., may be placed in one of the two categories—labored or inspired. Some

CREATIV

By RAY M.

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III. Genius *vs.* Insanity

Our method of calculating creative ability may at first seem to signify that insane persons would be the more creative. This is contrary to fact. In our estimation there are two types of disassociations, (1) creative and (2) neurotic. They differ in quality and degree. One may possess both. Lombroso's contention that genius and degeneration go hand in hand is obsolete and untrue. We prefer to refer to the similarities and speak of them as mere resemblances. "Genius resembles insanity as gold resembles brass" (Hirsch). Resemblances do not prove interdependence. At present we are planning to carry this same experiment into Psychopathic Hospitals and to determine the correlation between the normal individual and the neurotic individual with reference to their creative capacities.

Havelock Ellis tells us that "nothing in British Genius seems favorable to Lombroso's favorite theory, that genius occurs upon an epileptoid basis." "Some writers rake together cases of insane men of genius without considering what proportion they bear to sane men of genius, nor what relation their insanity bears to this genius."

The new combinations of thoughts of the insane have no significant bearing upon their past experience. Twelve deviations as expressed by a neurotic individual have an entirely different significance from the same number expressed by an individual with a sound mind. The two types fall under entirely different categories. The normal mind possesses the power of organization and the ability to realize the utility and function of its ideas while the insane person does not. Biography has warped the facts in the majority of cases of recorded insane geniuses. Freaks and seclusive types were formerly adjudged as insane. The meaning of the term has changed radically within the past two hundred years. The outstanding geniuses of today are far from being insane (Marconi, Voronoff, Edison, Carnegie). Let us hope that the influence of modern psychological methods will exact more careful consideration of mental traits in biographical records.

It is essential to record the "stability" of the individual with the results obtained by any test upon that individual. We proceed next to the test itself.

IV. Test for Creative Imagination (Visual)

Purpose. To determine the creative capacity of an individual.

Material. Four small round dots, representing the four corners of a square which measures ¾ in. from dot to dot, are printed upon white paper. (It is best to have a rubber stamp made in order that they may be uniform in each figure.) Five squares are printed upon an ordinary sheet of 8½ x 11 typewriter paper. Leave ample space about each square.

Preliminary. (For a group.) Pass out the printed material before starting to explain what is to be done. Give each individual 10 sheets of paper (50 squares). Have him write his name, age, and occupation in the upper right hand corner of the first page. If school children, have them give their grade in school. Have them lay papers aside while you give the directions.

Directions. (For a group.) "Upon each paper that I have given you there are printed several groups of dots in the form of squares. When I say 'Go' you are to add two more dots to the four, which you have in each of the printed squares, and see how many different drawings of objects, or of designs, you can make in fifteen minutes. You may place the two 'extra dots' anywhere you like. You must use every one of the six dots. You can make as many straight or curved or crooked lines as you like. Use pencil.

"Do not be too careful with your drawings. The objects must be intelligible and the designs or patterns must use the two 'extra' dots in important places in them. We know that some of you can draw better than others. That makes no difference. Originality counts for most. Strive to make as many *different* things as possible. You are searching for 'new' ideas. Try to make every one of the drawings *different*."

(Give two examples upon the black-board, showing how a dipper and a rectangle can be made by adding two dots and connecting them by lines. Be sure to make the dipper with a curved handle in order that they may see that curved lines can be used. Make it clear that they can use any kind of crooked lines.)

"Number each one of the drawings as you finish them. Remember to add two more dots to the four. You will have fifteen minutes only. Pay strict attention to your own business and do not look at your neighbor's work. The best way to find things is to keep looking at the four dots.

"READY! GO!"

(Watch the time closely. Be sure they have numbered the drawings before they hand them in.)

Questions and additional data:

1. Now some people place the two dots "by chance" anywhere within or outside of the square and then "imagine" what the outline suggests. Others first think of the thing they are going to draw, then place the two dots where they are needed to complete their picture or design. If the *thought* of the object you drew came into your mind before you placed the dots, write Yes on the back of the first page. If you used both methods write Both.

2. Write beside each figure what suggested it to you.

3. Did any of the things that you drew before have any influence upon your other drawings?

4. Did you think of the next figure that you were going to draw before you finished the previous one?

5. Which do you think is the more "creative" or original—the drawing of a geometrical wall-paper design or the drawing of an automobile?

V. How to Grade the Data

Give one point credit for a change of design in decorative drawings and one point credit for a change of object in representative drawings. By noting carefully the form, function, utility, shape, pattern, setting and meaning of each drawing it will be very easy to make these two main distinctions. Do not count a reproduction of the examples that were given before the experiment. Count only one letter of the alphabet and only one figure.

Geometrical figures count the same as any decorative design or representative object. The frequency of geometrical figures is about the same in all individuals. In talking with Professor Titchener about the comparative values of "designs" and of "objects," he stated that he was not certain but that the drawing of a design was more original and creative than the drawing of an object. We will not attempt to discuss this point here. We are inclined to think that they are of equal value. The artist may have a large supply of "stock" designs on hand to unload, but he is restricted to the use of only those which can utilize the two extra dots to advantage. A blacksmith, on the other hand, may have in readiness a supply of objects which he experiences with equal frequency in his work, but they are subjected to the same requirements in this test.

The total number of drawings made is not significant. If a person drew forty houses all practically alike he certainly would not be considered as "creative". Another may draw only five designs each of which has the same shape or form. We would score each with a zero.

Suppose Mr. X. made six drawings which were entirely different (man—house—boat—flower—fylfot symbol—butterfly), and Mr. Z. made thirty drawings of which only fifteen were entirely different. Mr. Z. would be considered more creative because he has given us fifteen changes while Mr. X. has given us only five. But what of the quality?

How are we to determine which is of more creative significance —a typewriter or a Rembrandt painting? How are we to differentiate between the creative abilities or creative capacities of a Goethe or a Schiller except by merely designating them as either active or passive? Who is the greater genius,—Plato or Bach? We are not dealing with qualitative distinctions in this test. We merely hope to ascertain quantitative creative values. The difficulties to be confronted in either case are extremely complex. The social significance or utility of a patent determines its value. We cannot go into a thorough discussion of this matter here, but will make that clear in a later paper. Quality depends to a great extent upon conditions external to the individual, upon education and environment. We are not testing the "product" but the "capacity" to produce original ideas. The fast thinker gives evidence of his speed in the greater number of figures that he draws, but bear in mind that there is greater chance of having many drawings alike where more are made than where few are made. College students in the Harvard Laboratory tended to make fewer figures and to get them all different, while the children in the grades draw a greater number of figures and get probably only half of them different.

Speed gives us no evidence. It merely signifies a difference

in method in creative production. While the fast thinker is drawing three or four similar drawings that only count for one point credit, the slow thinker is generally producing one figure that also counts for one point credit.

At first we attempted to collect the total number of figures made and unite it with the number of creative changes, stating both in a percentage. To secure this percentage we divided the number of creative changes (C.C.) by the total number of figures drawn. According to this method of figuring, an individual who made 5 figures, 3 of which were entirely different (60%), would make the same score as another who made 30 figures with 18 changes (60%). Here we have three creative changes in one individual equal to eighteen creative changes in another. Suppose Mr. A. made 15 figures with 14 changes (93%); Mr. B. made 22 figures with 21 changes (95%); Mr. C. made 15 figures with one change (6.6%); and Mr. D. made 22 figures with one change (4.5%). One can readily see that the value of the changes is here dependent upon the total number made. Our emphasis is upon the "changes", not upon the number of drawings.

VI. Value in Schools

In April (1921) this test for Creative Imagination was given to 407 children in the Public Schools of Oyster Bay, Long Island. Mr. B. E. Whittaker, Supt. of Schools at Oyster Bay, gave generously of his time and energy in aiding with the testing. The tests were given first in the eighth grade and then carried successively through the intervening grades down through grade 3 B. We started with the eighth grade in order to familiarize ourselves with the procedure before we approached the lower grades. Every effort was made to be sure that the procedure was well understood by all the pupils before starting. Language was adopted which they could understand. No essential deviation was made from directions as given above.

Following is a brief summary of the results obtained.

Grade	No. of pupils	Total No. of Figs. Drawn	Av. No. of Figs. Drawn	Total Creative changes	Average C. C.
3B	45	965	21.4	196	4.35
3A	39	721	18.4	276	7.05
4B	38	652	17.1	174	4.5
4A	39	734	18.8	379	9.7
5B	40	1067	26.6	349	8.7
5A	37	703	19.0	352	9.5
6B	34	578	17.0	372	10.9
6A	38	779	20.5	429	11.28
7B	36	566	15.7	349	9.6
7A	30	542	18.06	295	9.8
8	31	421	13.5	190	6.1
Total	407	7708		3361	
Average			18.9		8.25

These pupils ranged in age from 7 to 16 years. Each of the 407 tested had studied drawing. The finding which was of most importance to us was the average number of creative changes for the entire school. This average was 8.25. By comparing the number of creative changes of each individual with this average we have a basis for comparative judgments. Anybody who falls below this average is considered low in creative capacity, while anybody who ranks above 8.25 is rated accordingly.

In every grade, room A proved to be more creative than room B of the same grade. This seems to signify that Oyster Bay has a well graded school. Grade 6A has the highest average, with 11.28 creative changes. The budding genius of the 407 pupils was Edward M., 13 years old, in the sixth grade.[1] He drew 42 figures and had 27 perfect changes. Thirteen of his drawings were of objects.

These tests serve to indicate in a concrete way the latent capacities of each pupil. The task of the teacher then is to study the "interests" of the pupil and place him where he can develop his talents. If he is stupid in arithmetic, then try him in history. If backward in history, then try him in music or drawing. All must undergo a certain amount of the constraint of the school, however. It is evident that Edward M. possesses this "vital stuff" of life which we call "creative energy" and it is the duty of the teacher to direct its liberation into fruitful channels.

Those below the average can be of value to the community only as "reproducers" and probably will fare better in life than those above the average in creative imagination. Those above this average we term "producers"—those who bequeath something to civilization in services. The average business man who is successful may possess very little "creative imagination." True business ingenuity, however, lies in one's powers to visualize situations and in the creative impulse to react to productive opportunities.

VII. Development of the Test

At first we used 15 dots placed promiscuously upon a sheet of paper, and instructed the subjects to find as many "detailed objects or designs" as possible among them within 45 minutes.

[1] Mr. Whittaker says: "Many of those who made high scores may be classified as day-dreamers. Edward M. is about a 75% or 80% student in the usual school subjects. He is a special type and his case is difficult of analysis. He seems to have no strong likes or dislikes and no prominent interests, is not a movie fan, is not inclined to join with the other boys in vigorous plays, is not a great reader of stories or novels. Yet he has a very active imagination, exemplified in the peculiar kinks in his own expressions and ideas and his keen appreciation of new ideas expressed by other pupils. In the case of several others who made high scores, they are what one might call "shut-ins"—who for one reason or another have taken most of their play and recreation through imagination."

Three weeks of this procedure got us nowhere, because the setting was too complex and it was impossible to score the results. We then had the subjects make "one unified object" out of the 15 dots. Again it proved too complex for analysis, and eliminated creative design. We were also baffled with qualitative distinctions. There was only one thing left to do if we were to retain this method of approaching the problem, and that was to reduce the number of dots. Accordingly we took 6 dots and instructed the subjects to "make as many different drawings of designs or objects as possible by adding two dots to the six already given." The time was reduced from 45 to 15 minutes. This did not prove entirely satisfactory, because the six dots were too suggestive of definite contours of familiar common objects. To eliminate most of this we finally adopted the four-dot square as offering least suggestion and giving greater control in scoring. By giving the subject the freedom of placing the two "extra" dots anywhere he liked, and permitting him to use any kind of lines in making the drawings, ample opportunity was offered for the expression of his creative ingenuity.

The four-dot method is more tangible than a mere free association test, because it slows up the thinking process and gives more direct conscious attention to the evolving of a new idea. The pictured presentation of an idea gives us better ground for scientific investigation. We attempted to use the free association test by having the subject write as many words as possible within 15 minutes and then go through the list searching for the number of distinctly new ideas as they appeared. We had the subject go over his own list and state just where the breaks came in his thinking process while writing the list. Again we were swamped with complexities. There are certain possibilities, however, in this method. It is most difficult to ascertain the creative breaks in a list of free associated words.

All these preliminary experiments were given to four psychologically trained students in the department of Psychology in the Harvard Laboratory. One of the subjects was an artist and another was an instructor of Psychology in the Department. The other two were Seniors in Radcliffe College.

The artist proved to be most creative. She averaged 25.6 in the number of figures produced, and 22.8 in the number of "Creative Changes." The averages in the number of "Creative Changes" for the other three subjects were 15, 11.5, and 5, respectively. The number of figures drawn in 15 min. by these four subjects at their first trial with the four-dot method was 26, 18, 16, and 8. This gave an average of 17—being 1.9 lower than the average for the grade-school children given above. The number of "Creative Changes" for these four subjects at their first trial was 22, 15, 9, and 7, respectively. The average

of their "Creative Changes" was 13.2—being 4.75 greater than the average Creative Imagination of the 407 school-children. We make comparisons with the first trials only, because this eliminates any facility in performance gained through the subsequent trials. After four weeks' practice of one 15 min. period once each week, allowing the subject to repeat each week if he desired, we found that the averages were raised only about .5 in each case. This increase was almost constant in each subject.

VIII. Conclusion

To those who fail to recognize the value of this test as a method for the measurement of "Creative Imagination," we would merely suggest that possibly they could utilize the method to advantage in studying pure imagination. Our work thus far has been a play with method rather than with the solution of the problem. We believe, however, that our method is fundamental.

By joining a creative test such as we have outlined, with a "reproductive" test such as any general intelligence test, we shall get a more accurate statement of the worth of an individual. The intelligence test alone does not evaluate this vital "Creative Energy."

The test we have outlined deals primarily with a visual imagery stimulus to creative action. Probably some people would prove more creative in responding to an auditory stimulus. Some will remind us that this type of experiment does not draw out the potential "logical" creative capacities of an individual. We hold that it does. What could be more empty than four dots without any logical significance? It is true that we get an image of some object that we desire to draw, but the whole thinking process is involved in forming this image or association of neurograms. Visual imagery generally expands into scraps of kinaesthesis, auditory imagery and personal, organic or verbal references. Logical creative performance involves imagery. All imagery involves logical creative capacities. In the April issue of the American Journal of Psychology (1921), in an article by Claire Comstock on "Relevance of Imagery to Processes of Thought," we find the conclusion that "there is no irrelevant imagery."

This test gives equal chance of expression to the "dreamer of dreams" and the planner, the moulder and the constructor. The stream of imagination always flows in a determinate direction. One may think at first that a mere visual imagery process is of more value in finding artists than in finding lawyers. We must not fail to recognize, however, the transition which this "image" must undergo in being given symbolical representation in these drawings. The constructive mental mechanism of free association is supplemented by motor habit.

A TABLE FOR THE GRAPHIC CHECK OF THE METHOD OF CONSTANT STIMULI

By L. B. Hoisington

The graphic representation of the actual and of the theoretical values of p in connection with the Method of Constant Stimuli may serve as much more than a mere rough check upon the accuracy of the mathematical operations. The relation of the actual to the theoretical distribution of percentages, when presented graphically, reveals to the student just coming into psychophysical work the results of his rather arduous labors in a way which he can grasp. To tell him that the steepness of the curve determines the value of h, and that the 50% ordinate where it cuts the curve gives L in terms of the abscissa units, may mean more or less to him,—usually the latter. If he plots the curve of actual distribution alone his case is but little better; he must plot the theoretically best fitting curve over the actual. His knowledge of the use of the principle of least squares to get the best fitting curve is no more complete than before, but he can see that it does come out of his labors. With the two curves before him it is much easier to make him understand the principle of the law of error and the relation of a distribution due to 'sampling' to a distribution based upon an infinite series. It is also an easy way to show the relation of the ogive curve to the 'normal' curve of distribution, a relation which is by no means as evident to the beginner without mathematical background of the right kind as one is likely to assume. It serves as an excellent opportunity to show why the value of L is different from any one of the stimulus-values used.

For these reasons and also because it does serve as a check, rather rough to be sure, upon the work of a student, the instructor in a drill course often does well to plot the theoretical curve. Boring[1] gives directions for deriving the values and plotting the curve as well as the pedagogical advantages which come from its use. Also, as Boring points out[2], if we are to apply the principle of the sum of the squares of the differences as a test of adequacy, we must at least find the values for p_t and

[1]E. G. Boring, Urban's Tables and the Method of Constant Stimuli, *Amer. Jour. Psychol.*, 28, 1917, 288 and 291.

[2]*Op. cit.*, 288.

γ	p	γ	p	γ	p	γ	p
.00	.5000	.50	.7603	1.00	.9214	1.50	.9832
.01	.5057	.51	.7646	1.01	.9234	1.51	.9837
.02	.5113	.52	.7690	1.02	.9254	1.52	.9842
.03	.5169	.53	.7733	1.03	.9274	1.53	.9848
.04	.5226	.54	.7775	1.04	.9293	1.54	.9853
.05	.5282	.55	.7817	1.05	.9312	1.55	.9858
.06	.5338	.56	.7858	1.06	.9331	1.56	.9863
.07	.5395	.57	.7899	1.07	.9349	1.57	.9868
.08	.5451	.58	.7940	1.08	.9367	1.58	.9873
.09	.5508	.59	.7980	1.09	.9384	1.59	.9878
.10	.5563	.60	.8020	1.10	.9401	1.60	.9882
.11	.5618	.61	.8059	1.11	.9418	1.62	.9890
.12	.5674	.62	.8097	1.12	.9434	1.64	.9898
.13	.5730	.63	.8135	1.13	.9450	1.66	.9906
.14	.5785	.64	.8173	1.14	.9466	1.68	.9913
.15	.5840	.65	.8210	1.15	.9481	1.70	.9919
.16	.5895	.66	.8247	1.16	.9496	1.72	.9925
.17	.5950	.67	.8283	1.17	.9510	1.74	.9931
.18	.6005	.68	.8319	1.18	.9524	1.76	.9936
.19	.6059	.69	.8354	1.19	.9538	1.78	.9941
.20	.6114	.70	.8389	1.20	.9552	1.80	.9946
.21	.6168	.71	.8424	1.21	.9565	1.82	.9950
.22	.6222	.72	.8457	1.22	.9578	1.84	.9954
.23	.6275	.73	.8491	1.23	.9591	1.86	.9958
.24	.6329	.74	.8524	1.24	.9603	1.88	.9961
.25	.6382	.75	.8556	1.25	.9615	1.90	.9964
.26	.6435	.76	.8588	1.26	.9626	1.92	.9967
.27	.6487	.77	.8619	1.27	.9638	1.94	.9970
.28	.6540	.78	.8650	1.28	.9649	1.96	.9972
.29	.6592	.79	.8681	1.29	.9660	1.98	.9975
.30	.6643	.80	.8711	1.30	.9670	2.00	.9977
.31	.6695	.81	.8740	1.31	.9681	2.05	.9982
.32	.6746	.82	.8769	1.32	.9691	2.10	.9985
.33	.6797	.83	.8798	1.33	.9700	2.15	.9988
.34	.6847	.84	.8826	1.34	.9710	2.20	.9991
.35	.6897	.85	.8854	1.35	.9719	2.25	.9993
.36	.6947	.86	.8881	1.36	.9728	2.30	.9995
.37	.6996	.87	.8907	1.37	.9737	2.35	.9996
.38	.7045	.88	.8934	1.38	.9745		
.39	.7094	.89	.8959	1.39	.9754		
.40	.7142	.90	.8985	1.40	.9762	2.40	.9997
.41	.7190	.91	.9010	1.41	.9770	2.45	.9998
.42	.7238	.92	.9034	1.42	.9777	2.50	.9998
.43	.7285	.93	.9058	1.43	.9785	2.55	.9999
.44	.7331	.94	.9082	1.44	.9792	2.60	.9999
.45	.7378	.95	.9105	1.45	.9799	2.65	.9999
.46	.7424	.96	.9127	1.46	.9806	2.70	.9999
.47	.7469	.97	.9150	1.47	.9812	2.80	1.0000
.48	.7514	.98	.9171	1.48	.9819		
.49	.7559	.99	.9193	1.49	.9825		

take the difference between these and the actual values of p. The appended table is not sufficiently accurate for this purpose except in a demonstrational way, if indeed a demonstration may ever be less refined than what careful work demands.

In order to make the change from γ_t to p_t easier and quicker Dr. J. M. Gleason, as mentioned by Boring[3], computed from the table of Bruns the values of p_t for all the two-place values of γ up to 1.50 and from there on every tenth value. It is this table, slightly extended, which we here present in the hope that it may be of use to others, in drill-courses if not otherwise. For very accurate work one must still go to the more extended tables of Bruns and Kämpfe.[4] Our table is, in intent, in line with other tables, such as Rich's checking table,[5] the purpose of which is to reduce the mere mechanics of the work to the lowest possible limit consistent with the degree of accuracy which the task in hand demands.

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[3] *Ibid.*, 292.

[4] H. Bruns, *Wahrscheinlichkeitsrechnung und Kollecktivmasslehre*, 1906. B. Kämpfe, *Philos. Stud.*, 9, 1893, 147 ff.

[5] G. J. Rich, A Checking Table for the Method of Constant Stimuli, *Amer. Jour. Psychol.*, 29, 1918, 120-121.

ARE THERE ANY SENSATIONS?

By Robert M. Ogden, Cornell University

Are we not ready to dispense with the element of sensation in systematic psychology? Several lines of argument suggested by the present trend of psychological investigation lead to such a conclusion. The first of these is modern phenomenology. The second is the attack which has been made upon conscious entities by the behaviorists; and the third is the newer views of neurology, especially as they have been formulated by Henry Head and his associates.

I. The high points in the history of psychological experimentation, in so far as it has dealt with the nature of consciousness, have been, successively, the analysis of sensation, the investigation of memory, the attempt to bring the higher thought-processes under experimental control, and, lastly, the phenomenology of perception. In his account of the last Congress for Experimental Psychology which met at Marburg in April, Hans Henning tells us that a new psychology is now being born. "Until the turn of the century," he writes, "it was believed that one could grasp the mind with number and measure. This was the direction of the Wundtian School; however, few papers of this sort were to be found on the Congress programme; for since 1900 there has developed a qualitative psychology which concerns itself less with numbers and more with kinds of experience and qualitative analysis. One knows to-day that the complications and structure of experience can not be analyzed into simple qualitative elements and thus built up by joining one to another, but that one must work constantly with psychical forms (*Gestalten*)."

It is the investigation of these formulated structures that has led to a conception of psychological integration,[1] the elemental constituents of which are not psychical entities but aspects of an attributive order.

Even the earlier introspectionists were sometimes loth to admit that consciousness, when examined in cross-section, could be analyzed into so many entities, joined like the parts in a jig-saw puzzle. James, for instance, in his *Principles of Psychology*,[2] maintained that the taste of lemonade is some-

[1]See footnote next page.

[2]II, 2.

thing fundamentally different from sweet added to sourness, coldness, aroma, etc. The doctrine of mental chemistry, advanced to cover such synthetic discrepancies, was never entirely satisfying, because analogies of this sort are always inadequate to the factual data, and likewise for the reason that the chemical analysis of a compound into its elements is, indeed, exhaustive. The characteristics of water are the characteristics of its constituent parts, hydrogen and oxygen, and both these gases are susceptible of isolation and investigation. In the case of perception, however, analysis is more baffling; for the sensations therein contained reveal "aspects" such as extensity, intensity, and quality; and the entities or "contents" of which the perception is presumed to be made up can not be readily and individually isolated.

Although the logic of analysis is the same whether one analyse a material substance or a mental object, the constituents in the two cases are and remain distinct; and if it be the same objects of experience that give rise alike to the inferences of chemistry and the inferences of psychology, the inferences themselves are vastly different. One can carry over from one science to another the logical method and procedure, but never the precise point of view nor the existential data and technique of observation.

Perhaps we may regard the existential datum of all experience as a complex, having many facets, which, when viewed from diverse angles, reveals for each a unique phenomenological unit. It then becomes the problem of any special science to record the phenomena occurring under its view of the whole, and to infer the underlying order which constitutes the unity peculiar to its angle of observation.

Such an attempt was made in the older analyses of the conscious cross-section into sensations, images, and feelings. But the prejudice of quantification was too strong upon these earlier analysts; and their efforts were unsuccessful because an ex-

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[1]The use of the term *integration* is derived from the writings of H. J. Watt; *cf.* Some Problems of Sensory Integration, *Brit. Jr. of Psychol.*, 1910, 3, 323ff. "An intimacy of connexion between *nerve-paths* or impulses emanating from different sense organs is, of course, recognised in many forms. But this connexion has been somewhat exclusively considered to consist in a *mere coordination* or association of afferent and efferent impulses with one another. Sufficient attention has hardly been paid to the possibility that upon these afferent impulses an afferent structure might be raised which is dependent upon but essentially an addition to these. To distinguish it from mere coordination such a structure might well be called integration" (323 f). See also "The Elements of Experience and their Integration: or Modalism," *Brit. Jr.*, 1911, 4, 127 ff.; and "The Main Principles of Sensory Integration," *ibid.*, 1912, 6, 239ff., from which the following sentence is taken: "So the word integration may imply the general theory of the relation of a mode (*Gestalt*) to its basis in experience, which psychology may hope some day to attain" (247).

haustive procedure of weighing and measuring was obviously impossible. The mental object or phenomenon which furnished the point of departure was always existentially complex. However simple it might be rendered under the conditions of experimental control, it still persisted in remaining an object with a certain self-constituted 'thingness' about it. The greater its simplicity, the more definitely it might be described; yet the absolute simplicity of a single mental entity, to be taken up and examined apart from every other entity of mind, always eluded one's grasp. Were its color to be observed, the color must also have shape and size, or at least area; it must possess a certain brightness of intensity and a certain duration. Even a slight variation in one of these aspects might be sufficient to alter the entity so profoundly that it at once became something else.

As Titchener has pointed out in his paper on *Sensation and System*, "the sensation of classification is the logical resultant of many observations. Its qualitative attributes are (in the typical case) points selected by definite procedure from a continuum; its intensive attributes are themselves continua, reduced by the same procedure to a series of points. Hence, the conjunction of the qualitative attributes with any points whatsoever upon the correlated intensive scales constitutes, for classification, 'a' sensation; the bracketing together of the qualitative attribute with the complete intensive scales constitutes 'the' sensation. It would be a great simplification of psychology if a sensation, *tota, teres, atque rotunda*, would stand before us under a single comprehensive determination and allow us to observe it as a whole. But that, if it ever happens, happens only after we have made many separate observations of its distinguishable aspects."[3]

The question raised by my paper is one of doubt as to the genuineness of the hypothetical elements of sensation. Even since Titchener wrote in 1915 the investigation of the psychological nature of perception has revealed some of the characteristic integrations of attributes which constitute the fundamental patterns of attentional awareness. If a perception in its bare phenomenal essence is not found to be an aggregate of sensory elements, but an integrated unit, which upon analysis is reduced to a number of attributive aspects rather than to a number of individual conscious particles, it would appear that the elemental sensation can be experienced only under a determination of perception; and that, in so far as it can be perceived at all, it is a percept in the same way that 'movement' or 'wetness' or 'the taste of lemonade' is a percept. As Rahn states the case, "sensory consciousness is no simpler than, but equally

[3]This JOURNAL, 26, 1915, 261.

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[illegible] Psychology, *Psychological Review*

experience remains an abstraction of certain attributes which integrate to form a conscious *quale*, and this *quale*, under an appropriate attitude of observation, is as genuine an element of experience as any other. It therefore appears that the primary integration of attributes which determines the original units of sensory experience may embrace either more or less than the full complement which has been logically assumed to define the sensation.

II. The movement known as behaviorism falls outside the scheme of development which has accepted the analysis of consciousness as its chief task. It is nevertheless a logical consequence of the earlier efforts to measure mind, and the ease with which many psychologists have accepted the principles of behaviorism seems to indicate an inherent difficulty in the conception of sensation by the readiness with which the stimulus is accepted in its place. The radical programme of the behaviorist, which denies the scientific validity of all contents of consciousness, has contributed its part toward weakening the position of the conscious element by the destructive criticism leveled against the older notions of mental processes. However, in confining its investigations to stimulus and response, behaviorism has raised new problems which it must be prepared to face if the restricted means with which it proposes to operate in its investigations are to prove adequate in the solution of its problems.

Two points occur to one in which the means commonly employed by the behaviorist seem deficient in the attainment of his ends, the one being his failure to control the mediation between stimulus and response, while the other arises from inexact knowledge as to the nature of the stimulus itself. The dependence of the integrative patterns of perception, and likewise those of ideation, upon definite conditions of arousal from without and equally definite conditions and states of neural, muscular, and glandular receptivity and response from within, may, however, furnish that understanding of the mediation between stimulus and response without which we are so often at a loss to predict the behavior of an organism even under objective conditions that have been well defined and carefully controlled. As Boring has pointed out in his recent paper on the *stimulus-error*, "if only the end-terms of stimulus and response are controlled, a universal one-to-one correlation between stimulus and response is not possible"; and he goes on to say that the "only way out of the dark would be to study the effect of stimulation, of attention, and of criterion by taking hold of these dependent series at their intermediate points, thus providing ourselves with a more complete knowledge and control of the entire psycho-physical situation."[6]

[6] This Journal, 1921, 32, 449 ff.

as complex as, any [illegible]
its coming and goi[illegible]
other form."[4] [illegible]

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M[illegible]

that an impulse started in a specific end-organ travels unchanged to the highest receptive centre where it supplies the neural correlate of the sensory element of experience.

Still another point is suggested by Head's reference of affection to the optic thalamus. The anomaly of feeling as an element in consciousness has been frequently discussed; for feeling lacks the objectivity that belongs to the hypothetical elements of sensation and image. Yet the acceptance of *affection* as an attribute has been equally unsatisfactory, because it is not an invariable accompaniment of the elements, otherwise supposed to be fully described in terms of their attributes. If, however, we shift our mode of attack, and regard affection as a phenomenon of psychological integration, we may find it no more remarkable that we should be affected by certain integrations than that the integrations themselves should be compulsory in provoking distinctive psychological units of experience. Perhaps, too, the problem of the image would be better solved if we were to give up the attempt to discover its textural difference from sensation and limit the investigation to its phenomenological make-up as an integration of attributes taking place under certain definite conditions of experience.

In the programme of research here proposed we may still take general classes of sensation as a point of departure, though we should no longer be striving to reconstruct mental life as a certain number of conscious entities merely joined one to another in a mosaic of sensory particles. Rightly considered, even this analogy is more adequate to the facts of experience than was originally intended; for the mosaic has a pattern which the artist follows in the selection of his pieces of stone. Likewise, in the living organism, the pattern of the whole dominates the accretion of minor parts. To resolve the whole into its parts without regard to their relatedness in the original is to violate the principle of analysis. We do not go to the mosaic to study the nature of stones, but to study the nature of design. When the whole is broken up into its constituent pieces there is no design left. To attempt to understand the nature of mind by breaking it up into hypothetical elements is still more futile, because the elements themselves either disappear, or reconstitute themselves in a new type of experience which is a different subject of analysis from the one with which we started.

But if we take the perceptual pattern as our basis of analysis and study its attributive aspects under conditions which we can control, it may be that the varied integrations of these attributes will reveal to us the fundamental patterns underlying all experience. We shall be dealing with the only elements of mind that are capable of treatment in isolation one from another, and at the same time in intimate dependence upon physical

THE INFLUENCE OF CREATIVE DESIRE UPON THE ARGUMENT FOR IMMORTALITY

By Cavendish Moxon, M.A.

Human curiosity first serves the self-centered impulse to seek objects which cause pleasure and to avoid objects which threaten pain. In proportion to the growth of ethical repressions of crude impulse and cultural sublimations of infantile desire, curiosity has been diverted to problems in the external world. Civilization has involved a severe limitation of the open play of fantasy and the direct satisfaction of desire. Only by means of elaborate religious, artistic, and scientific disguises can the primitive desires escape. Indeed the aim of scientists is to discover fact even when fact contradicts the deepest desire.

Since many precious wishes have been hidden below consciousness, it is clear that the psychological study of the unconscious inevitably arouses great affective opposition. This is an obvious factor in some of the adverse criticisms of Freudian theories. In some cases the unconscious opposition is strong enough to prevent the severe renunciation of pleasure involved in accepting the new hypotheses. In other cases the admission of unwelcome facts has to struggle against the rationalizations and moralizations of primitive impulse and self-centered desire. The struggle is strongly marked in the artistic temperament which delights in new symbolic representations of unconscious desires and in imaginary association of pre-conscious impressions. The creative artist has a strong will to live which is symbolized by belief in survival of death. Moreover the artist tends to be conscious of the poverty of his expressions in comparison with the wealth of his impressions. He will therefore be almost overwhelmed by the smallness of his productions when he learns that all his previous experiences have been impressed and preserved in his subconscious mind.

Such, it would appear, is the feeling of M. Maeterlinck, who has recently published his views about the preservation of impressions in what he terms the subconscious mind. The poet accepts the facts which he uses as a new way of rationalizing the belief in survival which satisfies his deepest desires. The weight of these desires may be estimated by their power to

conditions that can be exactly measured. Thus there seem-
hope for a common meeting ground between the investigat
who by temperament and scientific interest insists upon limiti
his field of research to the tangible data of the physical univer
and the investigator who finds his problem restricted to
phenomena of consciousness. While a reconciliation is ha
to be expected, or even perhaps to be desired, it is neverth
of interest to the cause of science that all should work tc
a common end; for surely no gain can arise from a flat r
to admit the possible justification of another point o
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h he asserts as if it were an
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present writer. There spring
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which have no value for the or-
ole. No doubt such entirely use-
of behavior are exceptions. The
ctions in Nature have at one time
the species. The appendix was not
And the tendency of organisms to
tion of form or instinct usually pro-
it occasionally makes a self-destroying

argument would be stronger if he could
nscious impressions are useless for earthly
ne loss to be only partial—thanks to mem-
uition. Now if many seeds are wasted in
ay be used, the natural inference of a mind
nscious desire would be that a host of sub-
ons may be held in readiness to form a useful
utiful deed. M. Maeterlinck's premiss is so
invalidate his argument from the supposed
e subconscious impressions that a strictly util-
must provide another sphere of usefulness for the
ter death. Moreover, the usefulness of a sub-
ression is not wholly dependent upon its regaining
s. The psychoanalytical study of behavior has

made it probable that subconscious impressions when they become the images of desire indirectly play an important determining part in every act of judgment, choice, creation, and appreciation. The capability for attention, no doubt, strictly limits the recall of images; but M. Bergson showed forgetfulness to be useful for practical life. If memory were complete, choice and action would have an impossible task.

The biologist might be satisfied to have shown the weakness of M. Maeterlinck's argument. The psychologist's interest is by no means exhausted till he has gone a step further and explained the use of so weak an argument by so strong a mind. M. Maeterlinck is indeed only one instance of the paradox that eminent men often give illogical reasons for their belief in immortality. We must therefore generalise our enquiry and ask what it is that leads even men of genius to overlook the inadequacy of their reasons for this particular belief. The answer is given by the psychoanalysts, who have proved that men's reasons for belief in survival are unconsciously influenced by the desires they imperfectly conceal. The basic motives for the refusal to contemplate the annihilation of the ego are neither logical nor moral. The fact is, as Dr. Ernest Jones declares, that "in the unconscious every one believes in the omnipotence of his thoughts, in the irresistibility of his charms and in the immortality of his soul." The unconscious is not concerned with moral and metaphysical reasons for survival. The unconscious feels the primitive will to live. Death has no meaning for this level of the psyche, which refuses to think of the extinction of itself and of the objects to whom its love and interest have been transferred. In the unconscious dream-thoughts men appear to be alive many years after their death; time is abolished and the ego only "dies" or disappears in order to be reborn. The conscious arguments are therefore after-thoughts or rationalisations of the primitive wishes. The great unconscious weight of lowly psycho-biological desires accounts for the acceptance of many an argument, that, without this support, would be spurned by all intelligent men. Likewise the will to believe in survival without proof or *quia impossibile* is due to the unbounded egoism of the unconscious mind. Religious faith in the eternal moral values of the soul has its roots in the supreme worth of the unconscious ego in its own estimation; and belief in immortal life is a barrier against the fearful thought of wasted powers—powers which seem immense and are checked and limited by an indifferent world. A rationalistic psychology has supposed that the original cause of belief in immortality was the false reasoning of primitive man who believed he saw his dead friends alive in the world of his dreams. The new psychology, which seeks for wishes beneath thoughts, declares that

the savage saw his dead living in dreams because he could not conceive the intolerably unpleasant thought of the annihilation of those who ministered to the pleasure of his beloved and immortal self. The non-existence of time for the unconscious mind is proved by the fact that for many years after their death some parents continue to exercise a repressive and harmful influence over their family who only consciously accept the fact of their freedom, and show their bondage and fear in the dream-products of their unconscious life. An expression of the adult's unconscious refusal to face the fact of death sometimes openly appears upon the lips of the child. The following conversation reported by Dr. Jung makes this clear.

Anna, aged three, asks: "Grandmamma, why have you such faded eyes?"

Grandmamma: "Because I am old now."

Anna: "But that means that you will be young again."

Grandmamma: "No, I shall get older and older, you know, and then I shall die."

Anna: "Yes, and then?"

Grandmamma: "Then I shall be an angel."

Anna: "And then will you become a little child again?"

In night-dreams and to some extent in day-dreams is realized the desire of the self-centered psyche for a free and endless exercise of its powers in a perfect world. In metaphysical opinions and religious hopes the unconscious wishes have to compromise with the scientific interest in external reality. In men of poetical imagination like M. Maeterlinck, we see the fight of self-love for dominance, and the consequent imperfect rationalization of unconscious desire. M. Maeterlinck, we conclude, is led to use bad reasons in support of his belief in immortality because this belief is a necessary symbol of his unconscious desires, which dominate the thoughts in his conscious creed.

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[illegible] *[illegible] Allemagne* [illegible] supplément, [illegible]

2. [illegible] Ein Kapitel zum W[illegible] Lepzig, A. Kröner. Pp. [illegible]

3. [illegible] Eine Untersuchung der Entwicklungsgesetze von Sprache, [illegible] und Sitte. Vol. [illegible] Mythus und Religion. [illegible] Auflage. Large 8vo. Leipzig, A. [illegible] Pp. [illegible]

4. [illegible] Von Wilhelm Wundt und [illegible] With a portrait. [illegible] 8vo. Leipzig, [illegible] Pp. [illegible]

1916

1. Die Nationen und ihre Philosophie. Ein Kapitel zum Weltkrieg. [illegible] Small 8vo. Leipzig, A. Kröner. Pp. [illegible]
2. [illegible] Zu [illegible] Large 8vo. Leipzig, A. Kröner. Pp. [illegible]
3. *Elements of Folk Psychology: Outlines of a Psychological History of the Development of Mankind.* Authorized translation by E. L. Schaub. London, George Allen & Unwin, Ltd.; New York, The Macmillan Co. Pp. xxiii., 532.

1917

1. Völkerpsychologie. Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vols. vii., viii. Die Gesellschaft. Large 8vo. Leipzig, A. Kröner. Pp. viii., 438; vi., 434.

[1]The first instalment of this Bibliography appeared in the JOURNAL for October, 1908. Supplementary lists were published in the following October numbers up to and including the year 1914.

1918

(1) *Die Nationen und ihre Philosophie.* Ein Kapitel zum Weltkrieg. Kröners Taschenausgabe. 16th-25th thousand. Small 8vo. Leipzig, A. Kröner. Pp. 154.

(2) *Einführung in die Psychologie.* Vierter Abdruck. 19th-23d thousand. (Ordentliche Veröffentlichungen der pädagogischen Literaturgesellschaft "Neue Bahnen.") 8vo. Leipzig, Dürrsche Buchhandlung. Pp. vi., 123.

(3) *Grundriss der Psychologie.* Dreizehnte Auflage. With 23 illustrations. Large 8vo. Leipzig, A. Kröner. Pp. xvi., 414.

(4) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. ix. Das Recht. Large 8vo. Leipzig, A. Kröner. Pp. xviii., 484.

(5) *Das Reformationsfest.* Beiträge von Protestanten und Katholiken: Avenarius, Bonus, Eucken, Geyer, Gogarten, Hoffmann, Mumbauer, Natorp, Planck, Roethe, Rosegger, Tönnies, Troeltsch, Weinel, Wundt. Flugschrift 173 des Dürerbundes. Large 8vo. München, G. D. W. Callwey. Pp. 49.

1919

(1) *Vorlesungen über die Menschen- und Tierseele.* Sechste, neubearbeitete Auflage. With 53 illustrations. Large 8vo. Leipzig, L. Voss. Pp. xvi., 579.

(2) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. iii. Die Kunst. Dritte, neubearbeitete Auflage. With 62 illustrations. Large 8vo. Leipzig, A. Kröner. Pp. xii., 624.

(3) *System der Philosophie.* Vierte, umgearbeitete Auflage. 2 vols. Large 8vo. Leipzig, A. Kröner. Pp. xvi., 436; vi., 304.

(4) *Logik.* Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. i. Allgemeine Logik und Erkenntnistheorie. Vierte, neubearbeitete Auflage. With illustrations. Lex. 8vo. Stuttgart, F. Enke. Pp. xvi., 654.

1920

(1) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. x. Kultur und Geschichte. Large 8vo. Leipzig, A. Kröner. Pp. ix., 478.

(2) *Die Zukunft der Kultur.* Schlusskapitel aus Band x. der *Völkerpsychologie.* 8vo. Leipzig, A. Kröner. Pp. 54.

(3) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. iv. Mythus und Religion, erster Teil. Dritte Auflage. With 8 illustrations. Large 8vo. Leipzig, A. Kröner. Pp. xii., 587.

(4) *Die Weltkatastrophe und die deutsche Philosophie.* Beiträge zur Philosophie des deutschen Idealismus. (Veröffentlichungen der deutschen philosophischen Gesellschaft, herausgegeben von A. Hoffmann. Folge der Beihefte.) Sechster Heft. 8vo. Erfurt, Keyser'sche Buchhandlung. Pp. 16.

(5) *Erlebtes und Erkanntes.* Large 8vo. Stuttgart, A. Kröner. Pp. xii., 399.

(6) *Einleitung in die Philosophie.* Achte Auflage. Mit einem Anhang, etc. Large 8vo. Stuttgart, A. Kröner. Pp. xviii. 448.

(7) *Grundriss der Psychologie.* Vierzehnte Auflage. With 23 illustrations. Large 8vo. Stuttgart, A. Kröner. Pp. xvi. 414.

(8) *Logik.* Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. ii. Logik der exakten Wissenschaften. Vierte, neubearbeitete Auflage. With illustrations. Lex. 8vo. Stuttgart, F. Enke. Pp. xv. 671.

1921

(1) *Logik*. Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. iii. Logik der Geisteswissenschaften. Vierte, umgearbeitete Auflage. Lex. 8vo. Stuttgart, F. Enke. Pp. xii., 693.
(2) *Kleine Schriften*. Vol. iii. Large 8vo. Stuttgart, A. Kröner. Pp. vi., 549.
(3) *Probleme der Völkerpsychologie*. Zweite, vermehrte Auflage. Large 8vo. Stuttgart, A. Kröner. Pp. vi., 217.
(4) *Elements of Folk Psychology: Outlines of a Psychological History of the Development of Mankind*. Authorized translation by E. L. Schaub. [Revised edition.] London, George Allen & Unwin, Ltd.; New York, The Macmillan Co. Pp. xxiii., 532.
(5) *Die Nationen und ihre Philosophie. Ein Kapitel zum Weltkrieg*. Kröners Taschenausgabe. 26th-30th thousand. Small 8vo. Stuttgart, A. Kröner. Pp. 154.
(6) *Erlebtes und Erkanntes*. Zweite Auflage. Large 8vo. Stuttgart, A. Kröner. Pp. xii., 399.
(7) *Völkerpsychologie*. Eine Untersuchung der Entwicklungsgesetse von Sprache, Mythus und Sitte. Vol. i. Die Sprache, erster Teil. Vierte Auflage. With 40 illustrations. Large 8vo. Stuttgart, A. Kröner. Pp. xv., 695.
(8) *Metaphysik*. In Die Kultur der Gegenwart, etc. Vol. i., Abt. 6. Herausgegeben von P. Hinneberg. Dritte durchgesehene Auflage. Lex. 8vo. Leipzig, B. G. Teubner. Pp. x., 408.

NOTE.—The following publications have been transferred from W. Engelmann to A. Kröner:

1874 (2) *Ueber die Aufgabe der Philosophie in der Gegenwart*.
1876 (2) *Ueber den Einfluss der Philosophie auf die Erfahrungswissenschaften*.
1879 (1) *Der Spiritismus*.
1887 (6) *Zur Moral der literarischen Kritik*.
1901 (2) *Sprachgeschichte und Sprachpsychologie*.
1906 (4) *Essays*.
1907 (7) *Outlines of Psychology*.
1907 (11) *System der Philosophie*.
1910 (7) and 1911 (4) *Kleine Schriften*, vols. i., ii.
1911 (6) *Hypnotismus und Suggestion*.
1911 (7) *Naturwissenschaft und Psychologie*.
Grundzüge der physiologischen Psychologie. Sixth and last edition. 3 vols.
Völkerpsychologie. All current editions.

Magazines.—Wundt's *Psychologische Studien* ended with vol. x., whose title-page is dated 1917, though its final *Heft* appeared in 1918. The last three parts (Hefte 3, 4-5 and 6) receive the subtitles Königlich Sächsische Forschungsinstitute in Leipzig: Forschungsinstitut fur Psychologie, No. 1, No. 2, No. 3.

Wundt continued his consulting editorship of the *Archiv f. d. ges. Psychologie* to the time of his death in 1920 (vol. xl.).

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LVIII. An Experimental Investigation of the Experience Which Accompanies the Sudden Cessation of an Auditory Stimulus

By Elisabeth W. Amen

In his experiments upon the positive after-image in audition Bishop[1] found that a physically sudden cutting off of his stimulus-tones was not accompanied by a sharply ending tone for sensation. The transition from full tone through the changed character of the auditory experience to the ultimate disappearance of all sound of the tone he called the "modified ending." Throughout his experiments he tried to eliminate this modified ending, but was never successful. He concluded, at the end of his research, that the modified ending might be "due in part to tonal *Abklingen*, in larger part to the objective conditions" of his experimental arrangement.[2]

Problem

It is the purpose of this study to continue the investigation of the modified ending. Not every *O* had reported the modified ending at every stimulation. Only a few experiments were made by Bishop in which his *Os* were instructed to listen for it at every stimulation. These few led him to believe that it was always present, but the evidence supporting this belief was confined to the data obtained from a few experiments made at the end of his work. The fitful manner in which the modified ending made its way into the reports of his *Os* was the source of so much annoyance, that he sought relief from its irregularity of appearance in the possibility that it was really always present, and that his instructions to the *Os* had diverted them from it, so that they reported it only when it was sufficiently striking to force itself upon them. Our own first task, then, was to extend the number of experiments made under the instruction to listen for the modified ending at every stimulation, and thus to discover, if possible, what its existential character might be.

Experimental

Bishop's results indicated that the apparatus used in Series II[3] of his experiments could be utilised for our experiments. In that form it was simpler and more easily kept in adjustment than in some of his later forms. The only change from his arrangement in our first group of experiments was the omission of the sound-proof box. We decided that this would not be needed, because any echo would follow the final portion of the modified

[1]H. G. Bishop, "An Experimental Investigation of the Positive After-Image in Audition," this Journal, 32, 1921, 305-325.

[2]*Ibid.*, 325.

[3]*Ibid.*, 310.

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disposal we confined o
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Professor Josephine Gle
K. M. Dallenbach an
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Series I

given 30 stimuli in ha
We found that echo,
was a distraction,
sound-proof box.
the echo, reported some
was highly satis
of the nature of the
of the modified
pitch, intensity, and
phonetic sounds lik
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PITCH

S, flatting 8; M, flatting
M, flatting W, flatting 2, sh
M, flatting W, flatting 2, sh
W, flatting 0, sh

INTENSITY

M, no reference; W, greater
M, less W, greater
M, less W, greater less

VOLUME

no reference
M, no reference; W, no reference
M, less 4; W, less 3

DURATION

The modified ending is practically instantaneous, and mo
Occasionally, however, a very short or

This summary was made by counting the number of recurrences of any report by any *O*, and took no account of the fact that the stimulus-tones, presented to all *Os* at the same time, furnished another measure of the degree of agreement among *Os*. We found, upon investigation, that there was about the same amount of contradiction among *Os* whether we compared the reports as they are given in the summary or whether we compared the reports of all *Os* for the same tone.

Observation of the modified ending is very difficult; indeed, it is so difficult that we trusted to practice and the elimination of echo to bring the uniformity of report which we desired. Without further change than the boxing-up of the variator we proceeded with Series II.

Series II

In this series 60 stimuli were presented in haphazard order, 20 for every intensity. The same contradictions which we found in Series I appeared again; but practice worked a change in the kind of report, which altered the significance of the contradictions. In the beginning the modified ending was baffling to every *O*. It is very short; and, since it begins with a tone, there was strong predisposition to regard it as tonal and to assign tonal attributes to it. Our instructions favored this kind of report, though they did not exclude references to other observational characters which the modified endings might show. Reference to these other characters came earlier in the reports of some *Os* than in others, but the course of change within the reports of all *Os* was similar. They began, as we have indicated, with references to pitch, intensity, vocal character, and more infrequently to volume. Soon they began to mistrust their observations of pitch and said "doubtful rise" or "doubtful fall." Another development was the increasing observable complexity of the ending. It was not unitary but complex, and had a well-marked temporal course in spite of its brevity. It began with tone and ended with something more noisy, and during its course it seemed that the tone died away as the noise increased. This shift was expressed sometimes as a vocal change like oo-oop, and sometimes as an "aspirate" character or a "breathiness." Sometimes there was said to be a change in relative intensity or relative volume between the two phases of the ending. Everyone heard a decided change in the ending of every tone, but to give a verbal account of it severely tested every *O*'s descriptive and observational ability. The results showed further that the different *Os* attended to different aspects of the ending, and this fact produced heterogeneity of report even when there was the unifying factor of simultaneous observation of the stimulus-tones. The descriptions were so varied that unification into a typical modified ending for every intensity was well-nigh impossible. Every intensity gave the ending, and the stronger the intensity the more complex and more striking was the ending. Since it was our task to reduce the variability of the descriptive accounts, we made a second shift in technique for Series III, in which we used the weak stimulus only, on account of its simpler ending.

Series III

In spite of variability in what the *Os* said about the nature of the modified ending (57 stimuli in this series), there remains the more pertinent fact that some kind of transitional ending to the tones was always found. We desired first to secure evidence that tones were never cut off sharply for sensation, and secondly to find out what we could about the observable character of the ending. Throughout our study we have always found that the tones showed definite transformation as they died away.

Even though we did not find identity and full agreement in the descriptions, their very differences can be turned to good account. The

differences indicated that the *Os* either were not picking out the same aspects of the modified ending for report, or were observing under different degrees of proficiency due to different degrees of practice. To make a descriptive composite picture of the modified ending from the data at our disposal may be presumptuous; but we hazard such a picture, for it should be harmless, if it is regarded as a tentative construction, and should be useful as a stimulus to further experimentation.

There is a temporal course to the modified ending which begins with the first noticeable change in the character of the tone and continues until complete silence has set in. The verbal account will suggest that the modified ending is relatively long; but such a conception is highly erroneous; for its duration is of the same order as that of the click of a telegraph sounder. The pitch, which was clear and ringing during the tone, suddenly is encroached upon by a new quality which is said to be "breathy," "noisy," "hissy," etc. These two exist together, as flowing experiences, not static. The tonal part diminishes and the noisy part increases. The original tonal character to which the pitch was assigned very soon disappears. It is impossible, from the reports, to say whether it dies without change, whether it grows more shrill, as if it were present at a higher degree of attributive clearness, or whether it perhaps undergoes some other change. At this stage the experience is both tonal and noisy, and it is difficult to decide whether the tone-noise compound has a new pitch, or whether it is nonsense to speak of pitch at all. All agree that this stage is qualitatively different from the pure tone; and it is certain that the flatting or sharping reported is not the pitch-change of the musical slur. Following this middle stage, the tonal character diminishes to zero and the noise component reaches its maximum. The course of the noise begins with the breathiness which has already been mentioned and progresses into something hissy resembling the sound of *s* in *this*. It is very much softer and more breathy than *s* but is similar to it. The *s* passes over into a very soft, non-explosive, aspirated *p* similar to *p* in *up*, especially to that part of the *p* which is made by closing the lips as the breath comes against them; but not to the explosive part which accompanies the opening of the lips and the release of the compressed air in the mouth cavity. The modified ending could not be imitated accurately by any *O* in speech; but phonetic elements were easily recognizable in it, even if there was not full agreement about the letter which best expressed them. It is somewhat uncertain whether the tone continues until the end of the modified ending; some reports say that it does, others that it does not. In the matter of intensity there is also uncertainty. It seems probable that the general intensity of the modified ending is less than the intensity of the tone, although the weakening in the tonal component may perhaps be compensated by an increasing intensity of the noisy component. As regards the vowel quality of the tone, there is fairly general agreement that it is the phonetic *o*, or an admixture of another vowel with *o*. The clear vowel is, however, clouded by an admixture of the noise already described. Naturally, attributive volume has no significance if the pitch is constant. Reports on pure attributive volume were made much more infrequently than on other characters, if they were ever made. We are of the opinion that the coloring of noise rendered it impossible to judge pure tonal volume.

This, then, completes the picture which expresses the best founded generalizations from the results of Series III. It had to be pieced together from reports which were not the result of simultaneous attention of the *Os* to the same aspect of the tones. We hoped, therefore, to obtain greater uniformity by requesting them to attend in one set of observations to pitch, in another to vocality, and in a third to intensity. We devoted an hour's observation to each one, and finished the experimentation by a return for an hour to the instructions of Series III.

Series IV

Instructions: "Pick out the most striking pitch in the tone, during its course, and listen to it to see if it remains the same in the modified ending, or if it changes. If it changes, report the direction of change. If the pitch drops out before the end of the modified ending, tell what it is that follows."

The effect of this instruction was a gain only in this, that the reports made were on pitch. G said the pitch dropped; Br found that it sometimes rose, sometimes fell, sometimes remained the same; for D it generally dropped; for Bi it remained the same until it vanished. All alike reported an occasional lag of something breathy or aspirate after the tone had ceased. Since Bi is the most practised *O*, and since he too, during the earlier observations, reported a change in pitch, we suggest that more practice for the other *Os* might bring them to the same conclusion. H, who is a highly practised *O*, in the few observations which he made judged that the pitch dropped out, and that in so far as he could assign pitch to the non-tonal or less tonal aspirate part of the modified ending it flatted at the very end. This report is partial agreement with Bi, at any rate; the reports of Bi in some cases say that there is a different character at the end of the modified ending, which seems to be of lower pitch than the tone.

Instructions: "In this set of experiments you are to give your attention to the vowel quality. If there is a perceptible transition in the vowel quality from the ending of the tone to the ending of the modified ending, express this transition in phonetic characters; and describe it more fully, if possible, in whatever additional terms you choose."

Except for G, who said there was no change in vowel quality, every *O* reported a transition. For some it began with almost pure *o*, for others with about equal purity of *u*; it finished with something resembling explosiveness expressed by *p*, *h*, or *t*. Br inserted *e* and *i* during its course, but no other *O* indicated intermediate stages.

Instructions: "In this set of experiments you are to give your attention to intensity. Report the direction of any perceptible change in it, and add whatever additional description you can."

Again there was disagreement. The different *Os* did not agree either with themselves or with one another. All reported increase, decrease, and the same intensity. We suggest that the irregularity may be due in part to a confusion of intensity and attributive clearness. Bi reported "shrillness" which he never defined; but it seemed to resemble either a rise in intensity or an increase in clearness.

Return to Original Instructions. The added practice and the successive direction of attention to pitch, vowel quality, and intensity had a unifying effect upon the reports of pitch. Every *O* except D said that the pitch dropped out rather than changed; D reported, in several cases, that it was the same, with one report that it rose. In other respects the data taken in this last hour were typical of what has been said in the generalised description of Series III.

Conclusions. (1) We always found the modified ending of Bishop's experiments.

(2) The modified ending is certainly qualitatively different from the stimulus-tone; but we cannot be positive about changes in other respects.

LIX. The After-effect of Movement in the Sense of Touch

By Wellington A. Thalman

In his study "On the After-Effect of Seen Movement" Wohlgemuth reported a series of 34 experiments, the last one of which was conducted to see whether an after-effect of movement existed in the sense of touch similar to that in the sense of sight.[1] The result was negative, his conclusion being that "under the given experimental conditions, no analogous after-effect of movement exists in the case of touch."[2] In the theoretical sections which follow the experimental, this conclusion was further qualified: "the negative result of this experiment must not be considered as final; some observations made since then show me that the subject is worthy of further investigation."[3]

The present paper reports a series of experiments, which were undertaken in the interests of this problem during the Summer Term of 1921.

Observers.—The *Os* were Miss Catherine Braddock (*B*), fellow in psychology; Dr. Josephine Gleason (*G*), assistant professor of psychology at Vassar College; Dr. Karl M. Dallenbach (*D*), and the author (*T*). When the author observed, Miss Elizabeth Amen, graduate student in psychology, acted as *E*. All the *Os* were trained in introspection. They observed an hour a day, five days a week, and, in so far as it was possible, at the same hour every day. *B* and *G* worked without knowledge of the problem.

Experiments 1 *and* 2

We first sought to reproduce Wohlgemuth's experiments. He gives such a meager account of them, however, that instead of referring to his work we shall give a complete description of the method and procedure which we used.

Method and Procedure—A string-belt of cotton wrapping cord,[4] knotted at intervals of 4 cm., was driven over two horizontal drums at three rates of speed: slow, medium, and fast; moving respectively 7, 14, and 36 cm. per sec.[5] The moving stimulus was applied to the under side of the bare fore-arm, which was smoothly shaved so as to eliminate the drag and pull of the cord on the hairs. The fore-arms were alternately used, in order to exclude, as far as possible, the effects of fatigue and the after-images of pressure; consequently the direction of movement, which objectively was constant from the *Os'* left to right, was alternately ulnar-radial and radial-ulnar. The arm was placed between the two drums, directly over and at right angles to the lower warp of the belt. The hand grasped a support, and, at a given signal from *E*, *O* lowered his elbow to a padded rest, thus bringing his arm into contact with the moving stimulus. The hand-grasp insured the constancy of the place stimulated. To avoid fatigue the position of the hand-grasp, and consequently the position of stimulation, was changed between every two experiments with the same

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[1] *Brit. Journ. of Psych.*, Mon. Suppl. 1, 1911, 88, 109.

[2] *Op. cit.*, 88.

[3] *Op. cit.*, 109.

[4] We first tried a silk thread, the "thin silk cord" of Wohlgemuth, but abandoned it as the silk cut the skin and aroused complicating sensations of pain. In its place we used cotton twine, which, being larger and softer, did not have the saw-like effect of the other cord.

[5] Wohlgemuth does not state what speeds he used. He merely says that "various rates" were employed. We do not know, therefore, whether we duplicated his experiments in this respect.

arm. The areas stimulated were restricted to positions between 5 and 20 cm. from the first carpal folds. Three stimulation times, of 60, 120, 180 sec., were used.[6]

The apparatus was so arranged that the movement could be brought to an abrupt end, either with the cord in contact with the arm, or with it forced away from the arm.

In the first series of experiments the cord was forced away; in the second, the cord was allowed to remain in contact with the arm. In each series every stimulation time was used with every rate of movement and the whole repeated 5 times, making a total of 45 experiments for every *O*. The experiments were conducted in haphazard order.

Directions.—The directions used in these experiments were: "At the 'Ready' signal place your arm in position. At 'Now', close your eyes and lower your elbow to the padded rest. A continuous moving stimulus will be applied to your arm. When the objective movement has ceased and the resulting phenomena have run their course, give a complete account of them." These instructions brought out such a great mass of extraneous data regarding the sensations and perceptions aroused during the objective stimulation that the following was added. "Just before the objective movement is stopped *E* will give a second 'Ready, Now' signal. Give particular attention in your report to the phenomena, if any, which occur after the objective movement ceases."

Results.—Nothing was said in the instructions about after-images of movement; indeed, as we observed above, two of the *O*s worked without knowledge of the problem. We expected, nevertheless, since the *O*s' attention was directed to the interval immediately following the objective movement, that reports of negative after-effects would be given if such phenomena occurred. And such phenomena did occur. All *O*s reported instances in which the objective movement was followed by a movement in the opposite direction. The percent. of times these negative after-images of movement were reported is shown in Tables I and II. The results of the first series of experiments, those in which the cord was removed from the arm, are shown in Table I; the results of the second series, those in which the cord was allowed to remain in contact with the arm, are shown in Table II.

TABLE I

Showing the percent. of times that an after-effect of movement was observed when the cord was removed from contact.

	Rate	Slow			Medium			Fast		
	Time in sec.:	60	120	180	60	120	180	60	120	180
	B	20	40	40	20	60	40	0	0	20
	D	0	0	0	0	0	0	0	0	0
O	G	0	0	0	0	0	0	0	0	0
	T	0	0	0	0	0	0	0	0	20

TABLE II

Showing the percent. of times that an after-effect of movement was observed when the cord was allowed to remain in contact.

	Rate	Slow			Medium			Fast		
	Time in sec.:	60	120	180	60	120	180	60	120	180
	B	40	40	0	40	60	40	20	20	40
	D	20	0	0	20	20	0	0	0	20
O	G	20	0	0	0	20	0	0	0	20
	T	0	0	0	0	20	20	0	20	20

[6] Again, Wohlgemuth is not explicit; he merely reports "the experiments lasted from 1-3 minutes." *Loc. cit.*

In the experiments of Series 1, only two *Os*, *B* and *T*, reported the after-effect; but in Series 2, all *Os* reported it. The experiences were variously described as a "jerk backwards," a "slow movement backwards," a "diffuse drift backwards," a " backward wave-like movement," a "reverse movement," a "backward movement," and a "backward brush or sweep." The rate, extent, and duration of the subjective movement varied considerably: it was described as rapid and as slow; as large and as small; as of long duration and as of short.

The after-effect was noted more frequently when the stimulus was allowed to remain in contact with the arm than when it was removed; more frequently when the medium rate of movement was used than when either the fast or the slow rate was employed; and more frequently when the stimulus was applied for 120 sec. than when it was applied for the longer or shorter intervals.

We recognize that the negative after-image of movement occurred too infrequently in our experience to permit of our drawing conclusions regarding the conditions of its appearance. Our only claim is that our results demonstrate the existence of an after-effect of movement in the sense of touch analogous to that in the sense of sight.

Compulsory conditions were not obtained for any of the *Os*. The closest approach was with *B*, who reported the negative after-effect in both series of experiments in 60% of the cases when the medium rate of movement and the 120 sec. stimulation were used. That we did not obtain compulsory conditions we believed to be due to the fact that only small areas of the skin, areas approximately 1 mm. x 30 mm. in extent,[7] were stimulated. It is true, as our results themselves demonstrate, and as Wohlgemuth clearly showed in the field of vision, that an after-effect may be obtained when the objectively moving stimulus is confined to very small areas; but there is nothing in our results, nor, as we read Wohlgemuth, is there anything in his results,[8] that would lead us to believe small areas are as compulsory as large. In the endeavor, therefore, to obtain compulsory conditions and to examine the effect of size upon the illusion, we increased, in the following experiments, the dimensions of the stimulus.

Experiments 3 *and* 4

Method and Procedure.—In experiments 3 and 4 the width of the stimulus was increased by replacing the string belt by a muslin band 12 cm. wide. The apparatus used in Experiments 1 and 2 was otherwise unchanged.

The method of procedure, however, was changed. (1) Five stimulation times of 10, 20, 30, 60 and 120 sec. were employed instead of three; and (2) the number of repetitions was reduced from five to three. The stimulation times were increased in number in order that a greater temporal variation might be obtained and the effect of duration be more easily observed. The extreme stimulation of 180 sec. was omitted because we found that it was not as effective as the 120 sec. interval. The number of times the various experiments were repeated was reduced to three because of pressure of time.

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[7]The diameter of the cord was 1 mm. The length of the area stimulated varied for the different *Os*; 10 successive measurements for every *O* gave the following: *B*, 32 mm. $\pm$ 3.1; *D*, 39 mm. $\pm$ 3.7; *G*, 31 mm. $\pm$ 3.0; *T*, 30 mm. $\pm$ 2.5.

[8]One of Wohlgemuth's experiments—the 23rd—was undertaken "for the purpose of examining Exner's statement that an after-effect is only produced by moving surfaces of some size" (*op. cit.*, 73). Wohlgemuth used surfaces as small as 0.075 mm. x 5 mm. and found that negative after-effects were still produced. He did not, however, quantify his data, or compare the after-effect produced by small areas with that produced by large.

In the experiments of Series 3 the cloth band was removed from the arm when the objective movement ceased; in those of Series 4 it was allowed to remain in contact with the arm.

Results.—All of the *Os*, in both series of experiments, reported the negative after-effect of movement. The same descriptive terms were used as before.

In the experiments of Series 3, those in which the belt was removed from the arm, a new after-effect was reported, which, like Benussi's loop and bow movements,[9] seemed to leave the skin. It was described in such terms as: "movement away," "movement off," "movement from." We do not know whether these experiences were the result of the sudden formation of a negative pressure gradient, or of a stimulus-error; it may suffice at present to note that they occurred, and that they were not classified with the negative after-images of movement with which we are now concerned.

There was little difference in the results between the two series of experiments, or among the various speeds of stimulation. The differences among the stimulation times, however, were quite marked: the 10 and 20 sec. stimulations gave very few negative after-effects,—none in fact for *D* and *G*,—whereas the longer stimulation times gave the after-effect in 33, 22, 26, and 32 percent. of the experiments for *B*, *D*, *G*, and *T* respectively. At these longer intervals, the cases occurred about equally often.

The negative after-effect of movement was reported more frequently in these experiments than in those of Series 1 and 2, but still compulsory conditions were not obtained. An increase of width alone was not sufficient; therefore, in the following experiments, the length of the area stimulated was also increased.

Experiments 5 *and* 6

Method and Procedure.—The apparatus in experiments 5 and 6 was adjusted so that the stimulus could be applied longitudinally. A rest, supporting the arm at the hand and elbow, was built over one of the drums at an angle of about 45°, so that the rotating belt could be raised or lowered by means of a movable table pivoted at the lower end of the rest, and contact with the arm could thus be made or broken. The belt, 12 cm. wide, made of the coarsest corduroy obtainable, was driven at the same three rates of speed used in the previous experiments. The shaved under surfaces of the forearms were alternately used. The direction of movement was peripheral, from elbow to wrist. Three stimulation times of 30, 60 and 120 sec. were employed. The length of the stimulated areas was approximately 18 cm., the width, that of the forearm. In the experiments of Series 5, the table was lowered and the belt was allowed to fall away from the arm; in those of Series 6, the movement was abruptly stopped and the belt allowed to remain in contact with the arm. In each series every stimulation time was used three times in haphazard order with every rate, thus making a total in each series of 27 experiements for every *O*. The same instructions were used as in the previous experiments.

Results.—As in the other experiments, all *Os* reported the negative after-image of movement. More cases were reported in these than in any of the previous series. Their distribution shows that the stimulus was more effective when it remained in contact with the skin after the interruption

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[9] V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psych.*, 29, 1913, 385; Kinematohaptische Scheinbewegung und Auffassungsumformung, *Ber. ü. d. VI. Kong. f. exp. Psych.*, 1914, 31; Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psych.*, 36, 1916, 59.

of the objective movement than when it was removed; and furthermore, that there was little difference among the rates and the times of stimulation, —what little advantage existed was, however, in the direction of the faster rates and the longer times.

Though more reports of the after-effect were given in these experiments than in any of the previous ones we still failed to realise compulsory conditions. This failure we believed to be due to the inadequacy of the stimulation. The corduroy belt, on which we had set great hope, proved unsatisfactory. It was so soft and smooth, and the corrugations were so close together, that the *Os* frequently had difficulty in perceiving movement even during the objective stimulation; at times the movement adapted out and only a dull pressure remained; at other times, when the objective movement was perceived, the direction shifted.[10] It was not unusual for the *Os* to report that the direction had fluctuated 3 or 4 times during the course of a single stimulation. It was indeed surprising that so many reports of the after-effect were obtained under such poor conditions. In view of these facts, we decided to repeat the experiments with a more effective stimulus.

Experiments 7 *and* 8

Method and Procedure.—In experiments 7 and 8 a coarsely corrugated muslin cloth was used in place of the corduroy. The corrugations were made by sewing small pieces of cloth 2 cm. wide at separations of 4 cm. across the belt. The apparatus used in Experiments 5 and 6 was otherwise unchanged.

The method of procedure, however, was altered in two respects. (1) The direction of the movement was changed from peripheral to central; and (2) only two rates of stimulation were employed, which we shall designate, to bring them into line with the rates already used, as 'fast' and 'very fast'. The rate of movement during the 'fast' stimulation was 39 cm. per sec., approximately the 'fast' speed used in the other experiments; that during the 'very fast' stimulation was 109 cm. per sec., approximateiy three times the speed of the 'fast' rate. Otherwise the same procedure was followed as in Experiments 5 and 6. The instructions were not changed.

In the experiments of Series 7, the belt was allowed to fall away from the arm; in those of Series 8 the movement was stopped while the belt remained in contact with the arm.

Results.—The percent. of times that the negative after-effect of movement was reported in these experiments is shown in Tables III and IV.

TABLE III

Showing the percent. of times the after-effect was reported when the belt was allowed to fall from the arm, distributed according to the rate of the objective movement and the duration of the stimulation.

	RATE	FAST			VERY FAST		
	Time in sec.:	30	60	120	30	60	120
O	B	100	100	100	33	33	33
	G	33	33	67	100	33	67
	G	33	33	67	100	100	0
	T	0	0	0	67	33	100

[10]These experiences were analogous to those in the Bourdon illusion. *Cf.* B. Bourdon, *La perception visuelle de l'espace*, 1902, 176, 194.

TABLE IV

Showing the percent. of times the after-effect was reported when the belt was allowed to remain in contact with the arm, distributed according to the rate of the objective movement and the duration of the stimulation.

	Rate	Fast			Very Fast		
	Time in sec.:	30	60	120	30	60	120
	B	67	67	100	100	67	67
	D	67	100	100	33	67	100
O	G	33	100	100	100	100	100
	T	0	0	67	33	100	100

These tables show that compulsory conditions were obtained for all *Os*. Certain combinations of rate and duration, more or less peculiar to every *O*, invariably produced the negative after-effect. Though the distribution of the cases seems to indicate a tendency for the phenomenon to occur at the faster rates and the longer durations, we recognize that too few experiments were performed to justify us in drawing any general conclusions regarding these variables.

We feel justified, however, in concluding, from the data at hand, that the conditions are more compelling for the perception when the stimulus is not removed from the arm; that, in other words, pressure stimulation continued after the cessation of the objective movement is conducive to the perception of the negative after-effect. This conclusion is warranted not only by the fact that compulsory conditions were obtained more frequently when the belt was allowed to remain in contact with the arm than when it was removed, as shown in Tables III and IV, but also by the fact that more cases of the after-effect were reported in the experiments of Series 8 than in those of Series 7. Indeed, this conclusion is corroborated by the results of all our experiments: more cases of the after-effect were reported, as is clearly shown in Table V, in the experiments of Series 2, 4, 6, and 8, *i. e.*, in those in which the stimulus was allowed to remain in contact with the arm after the objective movement had ceased, than in those of Series 1, 3, 5, and 7, the series in which the objective stimulus was removed.[12]

TABLE V

Showing the number of times the after-effect was reported in the different experiments: those in which the stimulus was removed, and those in which it was allowed to remain in contact with the arm.

Removed		Contact	
Exp.	Number times after-effect reported	Exp.	Number times after-effect reported
1	13	2	26
3	19	4	28
5	17	6	28
7	40	8	53
Total	89	Total	135

[12]A total of 1080 experiments was performed. Half were performed with the stimulus removed, and half, under otherwise identical conditions, with the stimulus continued in contact after the objective movement had ceased. When the stimulus was removed the after-effect was reported 89 times, in 16 percent. of the experiments; when contact was continued the after-effect was reported 135 times, in 25 percent. of the experiments. This difference is too large to be due to chance.

These results accord with those of Wohlgemuth in the field of vision. Wohlgemuth found "that it is far more difficult to discover the after-effect in the subjective field of vision than in the objective. In other words, the after-effect is more easily discovered if the field of vision is filled."[13] By 'subjective', Wohlgemuth means the field of vision when the objective movement is interrupted by closing the eyes,—a condition comparable to the removal of the cutaneous stimulus; by 'objective', he means the field when the movement has stopped and the eyes are open and focused upon the fixation point,—a condition comparable to the retention of the static cutaneous stimulus on the skin.

Experiment 9

In order to ascertain the effect of movement upon some other area of the body a few experiments were conducted upon the calf of *T*'s leg. This part of the body was chosen as it was the only other area that could be stimulated without radically altering the apparatus. Mr. W. A. Andrews, a graduate student in psychology, acted as *E* during these observations.

With the exception that the direction of the movement was peripheral, from the knee towards the foot, the apparatus was used exactly as in Experiments 7 and 8.

Results.—The results of the previous experiments were confirmed. Negative after-images were again reported. As far as *T* was able to discern, the after-effect on the leg was as pronounced as that upon the fore-arm. These results, therefore, lead us to believe that an after-effect of movement may be obtained in the sense of touch from any part of the body that is adequately stimulated.

Experiment 10

After we had obtained compulsory conditions, a few experiments were performed in which the *Os* were asked to describe the negative after-effects in strictly psychological terms.

The apparatus and method used in Experiments 7 and 8 were again employed.

The instructions were altered to read as follows: "In the previous experiments you reported negative after-images of movement. You are now to concentrate upon the description of these processes. At 'Ready, Now' the objective movement will cease; when the resulting phenomena have run their course, describe the processes in strictly psychological terms."

Results.—An analysis of the introspections yielded the following results.

(1) The negative after-images of movement were variously localised by the different *Os*. At times the *Os* reported that the movement was cutaneous, at times that it was subcutaneous, and at other times a combination of the two. Excerpts, selected by way of illustration, are:

(*B*) "Creeping along *surface*."
"Movement uniform, smooth, weak, *subcutaneous*."
"Movement *cutaneous as well as subcutaneous*."
(*D*) "Light, filmy pressures which seemed to *envelop the arm* and float toward the wrist."
"Weak, light pressure, like puff of air moving on *surface of skin* with a duller, deader, *deeper*, more diffuse *pressure below the skin*."
(*G*) "Even pressure moving *along arm*."
"Flow of pressure *through* the arm."
"Bright, tingling pressure which moved *down the arm*, accompanied by a dull pressure which was *deeper* within the skin."

[13] *Op. cit.*, 31, 111.

(*T*) "Impression of movement floating *along* arm."
"Movement seemed to be *in*, *rather than along*, the arm."

(2) The quality of the after-effect varied with the localization. When localized in the cutaneous tissues, the after-images were described as "bright tickle," "light contact," "lively pressure;" when localized in the subcutaneous tissues, they were described as "vague," "dull," "dead," "deep," pressures. Examples are chosen from the reports of *D* and *G:*

(*D*) "After-effect of movement good.......... Quality of the after-images *bright, lively pressure*, and *deep, dull, diffuse pressure.*"
"*Bright tickle* sensations which moved along the surface of the skin."
"*Light* superficial *contact* which seem to float along skin."
(*G*) "After the objective movement ceased at first perceived a *bright, lively pressure* which seemed to shift toward the wrist; then a *vague, dull pressure* localized deeper within the arm."

(3) The interval between the cessation of the objective stimulation and the appearance of the after-effect varied considerably in length. At times it was very short, and the negative after-images seemed immediately to occur:

(*D*) (The objective movement was) "*immediately* followed by a short jerk backwards."
(*T*) "Movement perceived *as soon as* objective movement ceased."

At other times it was longer, and the negative after-images were slow in appearing:

(*D*) "Light, filmy pressures which seemed to........float toward the wrist, *slow* in appearing."
(*G*) "Dull pressures, seemed to shift toward wrist, came on more *gradually* than usual."

(4) The intensity, duration, rate, clearness and extent of the negative after-images likewise varied. At times the subjective movement was described as "intense" and "strong;" at other times as "weak," and "slight." At times it was reported to have "faded out very quickly" (*B*), to have been "of very short duration" (*D*), and to have "lasted just for a moment" (*G*); at other times it was reported to have "decreased slowly" (*B*), to have "long continued" (*D*), to have been of "long duration" (*G*), and to have "slowly faded" (*T*). The rate of the subjective movement was described as "rapid," "average or medium," and as "slow." At times the clearness was maximal, "as clear as if the cloth had been set going backwards" (*D*); at other times it was minimal: "the movement obscure, would not have observed it had I not been set for it" (*D*). The extent of the after-movement was at times definite: "the moving area was sharply and clearly defined" (*T*); but for the most part the extent was "diffuse," "ill defined," and "ill localized."

(5) The existential correlate of the perception of the negative after-image of movement appears to be an integration of spatial, intensive, and temporal aspects of the cutaneous or subcutaneous pressure sensations. The introspections upon which this statement is based are:

(*D*) "Slow backward movement along the surface of the arm. Bright lively pressure. The cutaneous sensations fluctuate spatially and temporally in intensity; by that I mean the cutaneous sensations, which are present over the whole area stimulated, rise and fall in intensity in a regular temporal and spatial sequence. The experience is clear, but very difficult to describe in attributive terms."
(*G*) "Dull pressure; movement gradual, seems to be spatial shift of intensity; quality did not change, it was the same dull pressure throughout."

"'Running' on arm. The 'running' is a rapid [illegible] ing in intensity."

Our analysis agrees very closely with that of Whitchurch given in her study on the illusory perception of movement on the skin. Whitchurch found that the perception of movement obtained by stimulating adjacent pressure spots could be described as "an integration of intensity, time, and cutaneous extent."[14] Our methods which differ in variety, the [illegible] are remarkably alike. If the difference is a true difference, it [illegible] may in one instance be a qualitative integration with time and extent, and in another an intensive integration with time and extent; then we have further evidence that the meaning of movement may be carried by several existential correlates."[15]

Summary

(1) Repetition of Wohlgemuth's work showed:

(a) that the apparatus was inadequate to the problem;

(b) that even under the unfavorable conditions of his experiments, all the Os reported an after-effect of movement in the sense of touch analogous to that in the sense of sight;

(c) that the after-effect was noted more frequently when the stimulus was allowed to remain in contact with the arm than when it was removed;

(d) that only a very small area of the arm was stimulated;

(e) that the apparatus must be modified, if compulsory conditions are to be realized, so that more effective stimuli can be applied.

(2) Experiments with the modified apparatus showed:

(a) that an increase of width alone is not sufficient to produce conditions compulsory to the after-effect;

(b) that an increase of width and length is still inadequate to the compulsory perception, if the stimulus is soft and smooth;

(c) that compulsory conditions are obtained when the fore-arm is longitudinally stimulated by a rough and coarsely corrugated linen band;

(d) that conditions are more compelling when the stimulus is not removed; that, in other words, pressure stimulation continued after the cessation of the objective movement is conducive to the perception of the negative after-effect.

(3) Experiments upon the calf of the leg showed:

(a) that the negative after-effect is as pronounced on the leg as upon the fore-arm;

(b) that the same general conditions obtained as upon the fore-arm.

(4) The experiments performed for the processual description of after-effect revealed:

(a) that the processes are at times cutaneous, at times subcutaneous, and at other times a combination of the two;

(b) that the quality varies with localization from "bright tickle," "light contact," "lively pressure," to "vague," "dull," "dead," "deep pressure;"

(c) that the interval between the cessation of the objective stimulation and the appearance of the after-effect is not constant; at times it is very short, at other times long;

(d) that the intensity, duration, rate, clearness and extent of the negative after-images vary; and

(e) that the after-effect is an integration of intensity, time, and cutaneous extent.

[14] A. K. Whitchurch, The Illusory Perception of Movement on the Skin, this JOURNAL, 32, 1921, 488.

[15] Op. cit., 489.

LX. Haptical Illusions of Movement

By William A. Andrews

The present study was suggested by Benussi's results in his experiments on the tactual illusions of movement.[1] Benussi, thinking to duplicate in the field of touch Wertheimer's work in vision,[2] stimulated successively two spots on the skin separated by distances ranging from 4 to about 170 cm. (the distance between the finger-tips with outstretched arms) and by times ranging from 160 to 2200 σ. He obtained, among other kinds of movement, a "bow" movement, which his *Os* described as the movement of 'something' up from the skin through the air and down to the second point touched, "eine Bogenbewegung in der Luft."[3]

The object of the present investigation was two-fold: (1) to determine the optimal conditions under which this kind of movement appears; and (2) to describe the experiences in strictly psychological terms and to identify, if possible, the existential correlates of the 'something' which moved away from the skin.

Observers.—The *Os* were: Dr. J. M. Gleason (G), assistant professor of psychology at Vassar College; Dr. Karl M. Dallenbach (D); and Mr. W. A. Thalman (T), graduate student in psychology. G and D were highly practised in cutaneous observation; T, though trained in introspection, had not observed before in a cutaneous experiment. G and T worked without knowledge of the problem other than that given them in the instructions. All the *Os* observed an hour a day, six days a week, and so far as possible at the same hour every day. The experiments were performed during the Summer Session of 1921.

We first sought to obtain the 'bow-movement' and to determine the optimal conditions under which it appeared. Our idea was to secure these results in a series of preliminary experiments and then, using the optimal conditions, to turn in the main experiments to the problem of description.

Preliminary Experiments

In the preliminary experiments the following instructions were read to the *Os* at the beginning of every experimental hour: "At 'Ready, Now' your forearm will be stimulated. Characterize fully the cutaneous perceptions aroused, using any common-sense terms you wish."

Apparatus and Procedure.—In these experiments the volar surface of the left forearm for D and T and of the right for G was stimulated by two successive pressure points. The arm was smoothly shaved and held comfortably in a fixed position by a plaster cast. A modified form of Benussi's kinohapt[4] was used in applying the stimuli. In order that no distracting temperature sensations should be aroused, the pressure points were made of hard rubber. The diameter of the rounded tips was 1 mm. An area of approximately 0.8 sq. mm. was therefore stimulated whenever the points were applied to the skin.

The stimuli were successively applied along (or parallel to) the longitudinal axis of the arm. The second stimulus was always peripheral to

[1] V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psych.*, 29, 1913, 385; Kinematohaptische Scheinbewegung und Auffassungsumformung, *Ber. ü. d. VI. Kong. f. exp. Psych.*, 1914, 31; Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psych.*, 36, 1916, 59.

[2] M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych.*, 61, 1912, 161 ff.

[3] *Ber. ü. d. VI. Kong. f. exp. Psych.*, 1914, 32.

[4] *Arch. f. d. g. Psych.*, 29, 1913, 385 ff.

the first. Before every experiment the pressure points were carefully set by a fine adjustment to a distance of 0.5 mm. above the surface of the skin. The current operating the kinohapt, which was controlled by a shunted rheostat, was checked several times during an observational hour. The strength of the current was constant; only a few times during the entire experiment did the milliammeter show a variation. Since the excursion of the pressure points and the strength of the current were constant, the intensity of the stimulus may likewise be regarded as constant.

The kinohapt was controlled by a Leipzig time-sense apparatus, driven by a Ludwig-Baltzar kymograph. The duration of each stimulus was 150 σ. The temporal intervals between stimuli were 100, 600, 1100, and 1600 σ; and the spatial intervals were 2, 6, and 10 cm. Each stimulus was applied but once; that is, there was but one application of the bi-membral stimulus during a single experiment[6]. The experiments were performed in series of 10. Every temporal interval was used in haphazard order with every spatial interval. At the end of 12 groups, which this procedure necessitated, the experiments were continued by reversing the order; the 13th series was like the 12th, the 14th like the 11th, etc. The practice-effect was thus evenly distributed throughout the series. Two precautions were taken to guard against fatigue: (1) the pressure points were moved to new areas after every experiment; we were careful not to stimulate the same point twice during a single hour; and (2) at the end of every series *O* rested his arm a few minutes by taking it from the cast. Usually three series of 10 experiments were conducted during an experimental period. Since 20 reports were given for every combination of the variables, a total of 240 reports was obtained for every *O*.

Results.—G and T did not report movement phenomena of any kind. Their experiences were described as two completely independent impressions. They characterized the impressions, localized them, mentioned the temporal sequence, etc., but never reported movement, or anything that could be interpreted as movement. D, on the other hand, reported the phenomenon in about 45% of the experiments. His movement reports were of three kinds:

(1) *Unimembral.*—One member was perceived as moving; sometimes it was the first, and sometimes the second. When the movement was at the first it was always in the direction of the second; when it was localized at the second, two types were distinguished: (*a*) the movement was in a peripheral direction away from the first; and (*b*) the movement was in a central direction toward the first. Examples are: "Two touches, second peripheral to first. First one seems to move toward second." (100σ, 6 cm.) "First one seemed to be stationary and second seemed to fly off." (1600σ, 6 cm.) "Second seemed to jump backwards toward first." (600σ, 6 cm.)

(2) *Bimembral.*—Both members moved. Two types were reported: (*a*) both members moved from points of rest; and (*b*) the first moved from a point of rest, and the second to a point of rest. Examples are: "Two touches which seemed to rebound from the skin." (1600σ, 2 cm.) "Touch left skin and landed peripherally about 3 inches away, partial loop, not complete at top of arc." (1600σ, 2 cm.)

(3) *Full movement.*—The movement was complete from the first point to the second. Two types were described: (*a*) the first impression "hopped," "skipped," or "jumped" to a new position; and (*b*) the first impression, "slid," "glided," or "was dragged" to a new position. Cases of the

[6] This procedure we knew to be at variance with Benussi's; we adopted it, however, as we thought that the repetition of the stimulus would give rise, through suggestion, to subjective conditions. We were desirous in these experiments of restricting our investigation to objective conditions.

second type occurred infrequently; only 6 cases were reported during the preliminary experiments. Examples of these two types are: "First jumped to new position, complete arc." (100σ, 2 cm.) "Touch which glided on surface of skin to new position." (100σ, 2 cm.)

The reports in which no movement occurred were of two kinds. (*a*) The impressions, though discrete in time, were localized at a single spot, as: "Two touches, seems as if the same spot was touched twice." (1100σ, 6 cm.) (*b*) The impressions were discrete in space as well as in time, as for example: "Touched twice in very rapid succession, points discrete, second peripheral to first." (100σ, 6 cm.)

Since the objective conditions were constant throughout the experiments, and since D and G were about equally proficient in cutaneous observation, we believe that the difference between the reports of D and those of G and T, in regard to the perception of movement, is due to a difference of attitude. It is certain that the subjective conditions were not the same: G and T, as we have said before, worked without knowledge of the problem; they approached the experiments naively; whereas D, who was familiar with the problem and knew the object of the present research, was set for movement. This fact was clearly established by one of D's early reports, in which he said, after characterizing the "bow" movement: "I am attending to the stimulus, I am set for movement, and an effort is made to obtain it." Under this attitude, which we shall call the "meaning" attitude, the objective conditions were at times sufficient to give rise to perceptions of movement; but under the attitude which G and T assumed, the objective conditions were never sufficient to produce the perception.

Though the object of the preliminary experiments was thus defeated, —optimal conditions for a 'bow' movement were not obtained,—the results are significant. They show that objective conditions are alone not adequate to the perception of movement; that we are dealing with a perception which is in part dependent upon subjective conditions;[6] and that we must establish the "meaning" attitude if we are to parallel Benussi's results.

Two methods of establishing the proper subjective conditions immediately suggested themselves: the one, a direct method, was to give the suggestion openly in the instructions by telling the *O*s what we wished them to attend to; the other, an indirect method, was so to cut the objective conditions that the suggestion would come from these themselves. For obvious experimental reasons the latter method was chosen for the Main Experiments.

Main Experiments

Procedure.—The procedure was altered in but one respect: the bimembral stimulus was applied in every experiment a number of times in rapid succession.[7] The time-interval between every two pairs was twice the length of the interval between each member of the pair. That is, when the temporal interval between the members of a pair was 100σ, the temporal interval between the pairs was 200σ, etc. We thought that this procedure would strengthen the association between the members of a pair, and that it would through expectation and habituation lead to the suggestion of movement. In order that the effect of this multiple stimulation might be the better observed and the formation of the meaning attitude (if such an attitude were formed) might be the better studied, the pairs were

[6] This agrees in substance with the conclusions of Whitchurch (A. K. Whitchurch, The Illusory Perception of Movement on the Skin, this JOURNAL, 32, 1921, 486 f.).

[7] This is evidently the procedure that Benussi used, though he is not explicit,—he does not tell us, for example, how many times the pairs were applied or what time-intervals elapsed between the pairs.

applied 5, 10 and 15 times in succession. Every temporal and spatial interval mentioned in the preliminary experiments was used 5 times in haphazard order with every one of these multiple stimulations: thus, a total of 180 experiments was performed for every *O*.

Instructions.—The instructions were extended to include process as well as meaning. "Two points on your arm will be stimulated in rapid succession. When I say 'now' (which was said after 5, 10, or 15 repetitions) (*a*) characterise fully in any common-sense terms you wish the cutaneous perceptions aroused and (*b*) describe the perceptions in purely psychological terms." 'Process' instructions were added partly because we thought that they would indirectly, as if by contrast, assist in the establishment of the subjective conditions necessary to the realization of the movement perception, and partly because we still hoped to obtain the existential correlates of the illusory movement.

Results.—Our attempt to establish thus indirectly the necessary subjective conditions was on the whole successful. All the *O*s reported illusory perceptions of movement. D reported movement of some kind in approximately 80% of the experiments, G in 92 %, and T in approximately 22%.

In addition to the perceptions reported in the preliminary experiments, D reported four new types.

(1) A new type of bimembral movement was reported, in which each point moved a short way in the direction of the other, as for example: "Two touches, each seemed to jump part way toward the other." (1100σ, 10 cm., 10 rep.) (2), (3), and (4). Three new types of full movement were reported. In one the movement was backward, from the second to the first; in another the movement was alternately forward and backward; and in the third the movement was double, two arcs diverging from one central point to two peripheral points. Examples are: ". . . . . Movement reversed and appeared to go backward, that is centrally." (600σ, 2 cm., 15 rep.) "Movement back and forth between the two points touched." (1600σ, 10 cm., 10 rep.) "Peripheral loop movement seemed to end at two different points." (1600σ, 2 cm., 15 rep.)

Though G reported movement phenomena in 92% of the experiments, she reported but two kinds of movements: unimembral movement, and full movement. Only two cases of unimembral movement were reported, and they were both of the same type; the first member moved in the direction of the second. These cases are: "Two touches, upper flicked off." (1600σ, 6 cm., 5 rep.) "Two successive touches, first flicked off in direction of the second." (1600σ, 6 cm., 5 rep.)

In 91% of the experiments G reported full movement. She distinguished four types. (1) The movement was from the first to the second; when of this type it took at times the form of an arc, bow or loop, as for example: "Pair of touches, hopping from point to point, always forward not backward." (100σ, 6 cm., 15 rep.). At other times it was on or in the skin as: "Movement peripheral, slid on skin to position of second touch." (100σ, 10 cm., 10 rep.)

(2) The movement was backward from the second to the first, as for example: "Direction of movement veered and went backwards. Backward movement does not come in until series has run for some time." (600σ, 2 cm., 15 rep.)

(3) The movement was alternately forward and backward, as: "Back and forth arc over arm touching it in two points." (600σ, 6 cm., 10 rep.)

(4) The movement was double, diverging in two arcs from a central point to two peripheral points. "Three touches, one central and two peripheral; meaning of movement from central to peripheral points in two diverging arcs." (1600σ, 2 cm., 10 rep.)

In the experiments in which no movement was reported, G characterized the experiences as "discrete temporally and spatially." In most

cases only two impressions were described, but in a few experiments three discrete points were reported, one central and the other two peripheral,—the static equivalent of the double movement mentioned above. "Three discrete touches, one central, two peripheral. One of the peripheral was like the central, the other was sharper. . No movement of any kind." (1600σ, 2cm., 10 rep.)

T, like D, reported three kinds of movement: unimembral, bimembral and full movement.

(1) Five types of unimembral movement were reported; in two the movement was localized at the first member, and in three the movement was localized at the second. When it was localized at the first member it at times left the skin, as: "First point jumped toward the second. Very definite, about half an inch." (100σ, 6 cm., 15 rep.) At other times it seemed to be in or on the skin, as: "Spread or slid down arm in direction of second." (600σ, 10 cm., 10 rep.) When the movement was localized at the second member it at times left the skin, as: "Jumping from the second toward hand, movement very definite." (600σ, 10 cm., 10 rep.) At times it seemed to be in or on the skin, as: "Second point seemed to creep on." (100σ, 6 cm., 5 rep.) At other times it seemed to penetrate the skin and go into the tissues beneath, as: "Second point appeared to move toward a third point deep under the skin."

(2) Two cases of bimembral movement were reported, in which both members of the bimembral stimulus seemed to move, as: "Both points seemed to float off the skin." (1600σ, 10 cm., 5 rep.)

(3) Though but 6 cases of full movement were reported,[8] two types were characterized. In the first the movement was in the form of an arc from the first to the second member, as: "Succession of arcs moving from the first to the second." (1600σ, 10 cm., 10 rep.) In the other the movement was subcutaneous, as: "Subcutaneous movement which went successively from the first to the second." (1600σ, 10 cm., 10 rep.)

The most favorable conditions of the illusory perception of movement differ greatly for the several observers. The optimal temporal interval is 100σ for D, 1100σ for G, and 1600σ for T; the optimal spatial interval is 10 cm. for D, 6 cm. for G, and 10 cm. for T. The only variable moment that the *Os* agree upon is repetition; a greater percent. of movement phenomena is reported with 15 repetitions than with either 10 or 5 repetitions, and a greater percent. is reported with 10 repetitions than with 5.

The conditions most favorable to movement in general are also, for G and T, the conditions most favorable for the bow or loop movement,[9] but for D a different array is shown: the optimal temporal interval for bow movement is 600σ as opposed to 100σ; and the optimal spatial interval is 2 cm. as opposed to 10 cm. As before, however, 15 repetitions are the most compelling.

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[8]In an effort to increase the number of bow movements, a supplementary experiment was conducted with T after the completion of the Main Experiments. The apparatus and procedure were the same as before, except that every temporal interval was used but once with every spatial interval, and that the stimuli were repeated 15 times in every experiment. The direct method of establishing the subjective conditions was used. We told T, in the following instructions, what we wished him to attend to: "Benussi obtained in his work upon the illusions of movement a loop or bow effect; that is, the first impression seemed to jump through the air to a new position, the position of the second stimulus. In the subsequent experiments, attend for this perception." Under these conditions T reported full bow movement in every experiment.

[9]This result would necessarily follow for G, since most of her reports were of the arc or bow type.

Compulsory conditions were not obtained for either D or T, but were variously obtained for G. No matter, within our limits, what temporal or spatial intervals separated the two stimuli, G reported movement in every experiment when the bimembral stimulus was repeated 15 times. G also reported movement in every experiment when the stimuli were repeated 10 times at the spatial interval of 2 cm. and the temporal interval of 1100σ, at the spatial interval of 6 cm. and all the temporal intervals, at the spatial interval of 10 cm. and the temporal intervals of 100, 1100, and 1600σ; when the stimuli were repeated 5 times at the spatial interval of 2 cm. and the temporal intervals of 100 and 600σ, at the spatial interval of 6 cm. and the temporal intervals of 600 and 1100σ, and at the spatial interval of 10 cm. and the temporal intervals of 100, 1100, and 1600σ.

The fact that there is so great a diversity in the results indicates that the perception of movement haptically aroused is not primarily dependent upon objective conditions. The essential requirements seem to be that the *Os* shall have the idea of movement and that this idea be given time for realisation. This conclusion is confirmed by the fact that the more frequently the stimuli are repeated, the more frequently are movement phenomena reported; and also by the fact that the *Os* in their introspective reports trace the gradual development of the perception from discrete points through unimembral and bimembral movement to complete or full movement. Examples are:

(D) "Bow is not complete at first, but with every successive stimulation it became more so until at the end it was complete." (15 rep.)

"At first bimembral movement. By concentrating on first member and by holding it in consciousness, complete arc movement resulted." (15 rep.)

"At start of experiment the impressions discrete; then first impression seemed to move, the second remaining static; then movement appeared at second; then movement was carried across in arc." (15 rep.)

"Character of movement changed during the course of the experiment. At first, first member moved; then second; then, forward arc movement; then backward arc, at which point the experiment ended." (15 rep.)

"At first bimembral movement. With successive stimulation the break or gap in the loop became smaller and smaller until just before the experiment ended the loop was completed." (15 rep.)

(G) "Discrete at first, then grouped in pairs, and then after a few stimulations got back and forth swinging notion of object touching arm at two points." (15 rep.)

"Meaning grew up after few stimuli had been given." (15 rep.)

"Two perfectly discrete touches; then the first one had a little tail which seemed to move on the skin in direction of second; then arc movement appeared......" (15 rep.)

"At first touches punctiform, then after a while got arc movement between the two points." (10 rep.)

(T) "Movement very definite toward end of experiment." (10 rep.)

"Very slight at first, became stronger toward end." (10 rep.)

The conclusion that haptical movement phenomena are subjectively conditioned is further borne out by the processual results. The dual instruction, to note meaning and process and to give a full report of both, laid a difficult task upon the *Os*, with the consequence that the descriptions of process were frequently sketchy and incomplete. Yet, sketchy and incomplete as the descriptions are, an analysis reveals the following:

(1) The arc, loop, or bow movement, whether unimembral, bimembral or full, is a meaning added to the primary pressure sensations.

(D) "Loop movement, high and rapid. Attending for the movement. When I attend to the sensations the movement disappears and I have only two neutral pressure sensations in consciousness. Movement best obtained by falling into stimulus-error."

(D) "Movement in high arcs. If I attend to the pressure sensations I do not get the arcs, just discrete pressures."

(G) "Movement in high arcs. If I attend to the pressure sensations alone I do not get the arcs, just discrete pressures."

(G) "Got the idea of movement in arcs simply through eye kinaesthesis. When I inhibit this and attend to the pressure sensations, get punctiform pressures, perfectly discrete."

(G) "Two touches and an arc movement between them. Movement is not given in experience, it is inferred. Course of movement can be imaged in either direction."

(G) "Back and forth movement, inferred from visual and thoracic images. As far as experience goes there are simply two pressures."

(2) The processes which carry this meaning differ for the different *Os*, and for the same *O* at different times. At times the accruing processes are visual, at times they are kinaesthetic, and at other times a composite of the two, as the following examples show:

(D) "The pressure sensations are accompanied by visual images of line-like loops which are grey in quality like the grey of a pencil mark on white paper. This grey is weak at first, but it becomes stronger and more distinct as the experiment progresses. The bow was not complete at first, but with every successive stimulation it became more complete, until the arc was united in imagery. Visual images became clearer and more complete. Coordinate with this, vague and indefinite kinaesthetic sensations or images of head and eyes moving back and forth in rhythm with the stimulation."

(D) "Complete loop, very high and rapid. The cue for movement came from the changing pressure gradient and the meaning given by the visual and kinaesthetic imagery which supplemented it. The visual imagery was of a loop greyish in quality, much like the grey mark of a pencil on white paper. Kinaesthesis of eye movements along arc of this loop; think there were also kinaesthetic movements of nodding of head in rhythm of movement."

(G) "Discrete at first, then grouped in pairs, then peripheral arc movement. Positive of movement, it is there, but there is no sensory basis; given entirely by visual image of an arc with its terminals on the skin."

(G) "Backward movement, 2-1, meaning carried by eye-movement and perhaps thoracic pressure."

(G) "After series had gone on for a little while I got notion of movement in arc, large loops between terminals, carried by thoracic pressure and eye movement."

(G) "At first two perfectly discrete touches, did not think of object making them. After series ran awhile got movement between the two points. Principal thing was eye-movement, tracing arc in imagery."

(3) When the movement is in or on the skin the perceptions appear to be of a different kind; the meaning is inferred from the pressure sensations themselves, and the existential correlate seems to be a spatial and temporal integration of pressure.

(D) "Seemed to slide along skin..... a rapid peripheral extension of the pressure quality."

(G) "Slid on skin, change in extent of experience in time."

Summary

(1) We discovered in the Preliminary Experiments, which were undertaken to identify the 'bow' movement and to determine the optimal conditions under which it appeared, (*a*) that our objective conditions were alone not adequate to the perception of movement; (*b*) that we were

dealing with a perception which is in part dependent upon subjective conditions; and (*c*) that we should have to establish the 'meaning' attitude if we wished to continue the investigation.

(2) We found in the Main Experiments, in which we endeavored to establish the necessary attitude indirectly by rapidly repeating the bimembral stimulus, and by asking in the instructions for reports of process as well as for reports of meaning, (*a*) that various kinds and types of movement, unimembral, bimembral and full, were reported by all the *Os*; (*b*) that the optimal condition for the arousal of the perception varied greatly from *O* to *O*; and (*c*) that the diversities as well as the uniformities in the results of the different *Os* indicate that the perception of bow-movement haptically aroused is not primarily dependent upon objective conditions. The essential requirements seem to be that the *Os* shall have the idea of movement and that this idea be given time for realisation.

(3) The analysis of the introspective reports corroborates these conclusions. (*a*) The perception is gradually built up; (*b*) the arc, loop, or bow movement is a meaning added to the primary pressure sensations; and (*c*) the processes which carry this meaning are associated visual and kinaesthetic images, or incipient sensations.

(4) When the movement is in or on the skin the perceptions appear to be of a different kind. The meaning is inferred from the pressure sensations themselves, and the existential correlate seems to be a spatial and temporal integration of pressure.

SOME RECENT PSYCHOANALYTIC LITERATURE

By G. Stanley Hall

A Young Girl's Diary. Anon. Pref. by Sigmund Freud. N. Y., Seltzer, 1921. Pp. 284.

This diary was begun by a Viennese girl of eleven, and continued until she was fifteen and a half. She belonged to the well-to-do and intelligent bourgeoisie, and was evidently a girl of a somewhat precocious type, not unusual in our day. The most remarkable thing about these very secret and confessional records of incidents and spontaneous feelings is that so large a part of all the child's interests, gossip, and activity was to find out more and more about the sources of human life. Every item of this knowledge which filtered in was pooled with that of her most intimate chums, reasoned about, and made the center of all kinds of emotional activity, so that the volume might almost have been entitled A Girl's Four-Year-Old Struggle to Understand Sex and Reproduction. It all illustrates in a rather too exquisite way Freudian theories of the immense travail of soul involved in the *Aufklärung* and we have hints in it of about all the aberrations of the *libido* which are described in his writings. Freud says that nothing has ever been written "enabling us to see so clearly into the soul of a young girl during the years of pubertal development." "The little author is a literary artist." In the diary, he adds, we see how interest in sex-life first arises vaguely and "then takes entire possession of the growing intelligence, so that the child suffers under the load of secret knowledge but gradually becomes enabled to shoulder the burden."

Scores of entirely innocent and neutral words (secret, figure, understanding, illness, disease, relation, period, part, and many more) suddenly become centers of intense self-consciousness and curiosity, common knowledge of these being the basis of friendship and its impartation being friendship's chief function. From a sense at first of being shut out from all that made life really interesting, this child, by incessant prying and searching, slowly came to feel that she had a very superior knowledge and even had a mission to initiate others. An adult will be amazed to find how many partial faults and misleading concepts are possible in this field. If this is the most important of all kinds of knowledge, this girl was a genius and a superb psychologist without knowing it. She penetrated to, and gave away more completely than anyone else, the secret of her sex during the period of its most rapid development.

The questions that will inevitably arise in every reader's mind are: first, whether the child was normal; and secondly, whether her surroundings in Vienna did not bring her to very unusual envisagement of all sorts of improper things. In any case, the book is a remarkable contribution to "flapper" psychology, of which we know so very little, and takes its place beside the revelations of Marie Bashkirtseff, Mary MacLane, and Una Mary. It should be added that, if the book were radically expurgated, there would be enough left to make very interesting and stimulating reading for girls of like age and their parents, although it would be greatly reduced in size. But as it stands, it would be a grave mistake to allow

it to fall into their hands. While cultivated parents of neurotic girls of this age might be helped by it, its circulation should be chiefly among the medical profession. The expurgation of a dozen or a score of pages would greatly help.

The Psychology of Daydreams. By J. VARENDONCK. Introd. by SIGMUND FREUD. Lond., Allen and Unwin, 1921. Pp. 367.

Dr. Varendonck, a brilliant young Belgian student best known by his study of children's societies, was for three years during the war an interpreter for the Allies and writes his book in English. He had read little but Freud's "Dream Analysis" before he entered the war, but after some years succeeded in getting hold of the mode of thought-activity which has been called autistic or fore-conscious or, by Jung, undirected thinking, which is best studied just before going to sleep. The moment he becomes aware of these dreamy states he concentrates upon the last link of the chain, and by dint of long practice has been able to drag up previous links, so that he gives us, based very largely upon his own personal experience and self-study, by far the best picture of revery, which shows inner mental life in its estrangement from the outer world. Hysteria is the invasion of the system of motility by unconscious reveries. The paranoid gives these reveries reality. The productions of revery are much more accessible because the inner self does not drift so far from its outer conditions. In the early part of the book the author has, to his mortification, to give us many personal details that we may understand his reveries, but later these are supplemented by the reveries of others and the personal element fades. These studies convince him that voluntary thinking is a recent adult acquisition, and that in revery we are thinking as the child or as primitive man thought. In conscious thought we are able to eliminate affective processes, but these dominate in the fore-conscious. The latter, again, has a very highly developed symbolic character and can never be abstract. It is also egocentric and is rarely entirely impersonal. Like dreams, reveries often center about unsolved problems and cares, and their end-exteriorization is of a more immediate and topical character. They are almost always adaptive and in a sense teleological. And yet revery and play, as interpreted by Groos, have much in common. The censor is less active than in directed thinking, but more so than in dreams. Conscious activities are all assisted, or should be, by affective thinking. Conscious thought, however, is characterized by far greater freedom from the defects engendered by emotions, and should perhaps be characterized as the elimination of all affectivity. It is under the dominion of volition only. It alone can be truly speculative. The author's conclusion is that unconscious, fore-conscious, and conscious thinking are three manifestations of the same process varying only in degree of function which, originally regulating the relations of the individual with the outer world, constitutes a manifestation of universal energy and is as eternal and unceasing as the other organic activities in the service of adaptation.

The New Psychology and Its Relations to Life. By A. G. TANSLEY. Lond., Allen and Unwin, 1920. Pp. 283.

This book claims to be the only one in English which has attempted to gather all the light shed by psychoanalysis upon the behavior and treatment of normal individuals. All the factors characteristic of the mentation and behavior of the neurotic are at work in the normal individual, whom the analyser does not see and whom he too rarely considers. This gap the author seeks to fill by giving what he calls a biological view of the

mind, with selections from the literature that was most helpful, but with no attempt to deal with the psychopathology proper. Accordingly, after an introduction briefly characterizing "the new psychology," the physical and psychical worlds, he passes in Part II to the structure of the mind, specific responses, typical mental processes, the unconscious, and complexes. Under "mental energies" he discusses *libido*, which he identifies with interest, equilibrium, and sublimation. The byways of the *libido* are suggestibility, failure and regression, conflict, forgetfulness and repression, dreams, projection and idealism, psychical segregation and displacement. Part V treats of reason and rationalization and its relations to conduct; while the last Part, dealing with the contents of the mind, characterizes the primitive instincts, the great complexes, especially that of the ego, the partial and universal herd, the sex instinct and the primary sex complex, byways and combinations of the sex instinct, and the interpretation of the universal complexes. As a whole, the work is undoubtedly the best introduction to the subject for the general reader whose interest is not primarily in abnormalities.

Psychoanalysis and the War Neuroses. By S. FERENCZI, K. ABRAHAM, E SIMMEL, and E. JONES. London, 1921. Pp. 59.

There has been a very strong and growing conviction among the great majority of physicians who dealt with the psychic traumata of the great war that the sex factors on which the Freudians laid so much stress had little or nothing to do with the causation or the cure of these cases, but that they were purely of ontogenetic origin and due to fear, conscious or unconscious. This little volume, with an introduction by Freud, attempts to convince us that this view is erroneous, that the neuroses of war and peace are not fundamentally different, and that obscure sex factors enter even into shell-shock, in various cases of which we have "genitality" shown in various symptoms. War-neurosis is simply a traumatic neurosis such as was well known to occur after fright or severe accidents without any reference to an ego conflict. The *libido*-theory was put forward by Freud only with reference to the transference of the neuroses from peace conditions.

Ferenczi's article in this symposium is extremely valuable as containing a survey of all the very voluminous German literature bearing upon the subject. Indeed, in no other of this author's writings known to us are we so impressed with his breadth of view and knowledge and his mental activity in coming to terms with so many different shades of conclusion. He shows that many German specialists have accepted very many of Freud's conclusions apparently without knowing it, and that despite the violent opposition of the Oppenheim group. Even the marked regressive character of all war neuroses, shown at the conclusion of peace, had been described by Freud before the war, although he was speaking only of accidents.

Abraham in his contribution thinks that the war traumata act on the sexuality of many people in the sense that they give the impulse to regressive alteration which endeavors to reach Narcissism. The soldier must always be prepared for unconditional self-sacrifice in favor of the mass, and this signifies the renunciation of every vestige of Narcissistic privilege. In the unconscious of many we do not suspect of Narcissism slumbers a belief that they are somehow invulnerable or immortal, and an explosion or wound suddenly destroys this belief, so that the security they felt collapses into a feeling of powerlessness and then the neurosis sets in.

The best of these papers is by Simmel, who has really nothing to say of the *libido*-theory, but describes his own war-experiences when he was in charge of a special hospital for the war neuroses, and finds Freud's

views in regard to the unconscious psychic causation and cure to be abundantly confirmed. The victim takes refuge in his symptoms, not for the purpose of preserving his physical but his psychical existence. There are very few war psychoses but very many neuroses. The conditions of the soldier's life involve the constant narrowing of his ego complex. He is an inconspicuous unit in a vast whole, must have no will of his own, and the narrowing and suppression of his consciousness represent the initial stage of the war neurosis and consciousness may be lost suddenly. Then the unconscious has its innings. By hypnosis he can be made to live through his experiences, and this was found to be very effective. Doctors who devise systems of torture, hunger cures, dark rooms, prohibition of letters, painful electric currents, etc., to compel patients to abandon their neurotic symptoms, really recognize Freud by inversion of his fundamental principles, *i.e.*, they make the patient wretched to force him to flee into health.

Jones' articles is more controversial, and is a defense of Freud's theory of the neuroses. War itself is an explosion of forces that are in conflict with the standards of civilization. "It is an official abrogation of civilized standards" sanctioning barbaric activities. He tells us that the readjustments necessary in war are "by no means so difficult as can arise in various situations appertaining to the field of sex." He takes his departure from Narcissism, and suggests that not only sex suppressions involved in war but wounded self-love, the severity of discipline, the imminence of danger and even death, and fear, which is the thing centrally to be considered (because, as Freud teaches, all psychoneurotic symptoms are constructed to prevent the development of fear and anxiety), will be found, when we have fully understood the war neuroses, to be the key to the explanation of all symptoms.

A Psychoanalytic Study of Manic-Depressive States. By Lucile Dooley. Psychoanalytic Rev., 8, no. 2, April, 1921.

This is a very interesting and critical study of five cases of a mental symptom-group which it has generally been supposed psychoanalysis cannot help; but the author concludes that, while in general the psychoanalytic results have been meager and doubtful, there was material assistance, especially in three cases, which had not however become chronic. In one there were real alternations in cycle although there was little likelihood of permanent recovery. In another, a bigoted, self-willed character, there was little help because the patient did not cooperate. None of the cases had much intellectual training, and this is very important, as has often been pointed out, especially for securing the needed attention and interest. Four out of the five cases had reached puberty at an unusually early age, and all had developed sex repressions as a result of the mother's failure to meet their needs at the critical time. There was unsatisfied curiosity, doubt, and fear before twelve years, when the patients were unable to meet their problems without help. Thus all four patients who married did so with lack of self-control, excessive bashfulness, modesty, prudery, incipient homosexuality, so that their marital relations were unhappy. Their delusions were usually attempts to fulfill regressive wishes, and it was possible to trace the stages of regression step by step to deeper and deeper layers of the unconscious. Thus the manic-depressive type does seem, contrary to the usual conceptions, sometimes to descend to levels as low as those reached by dementia praecox. The manic-depressive character is extroverted, always trying to relate itself to the environment, but minimizing the subjective element. The behavior of manic attacks is evidently a defence reaction, and in a depressed phase offence is no longer possible under profound consciousness of defect. Hence the difficulty of psychoanalytic treatment. The patient cannot bear to hear the truth.

Dangerous Ages. By Rose Macaulay. N. Y., 1921. Pp. 242.

This novel has a unique psychological scope and importance, describing typical women of four generations living together at the ages respectively of just over 20, 43, 63, and 84. The great-grandmother, of the latter age, is made an oracle of sense and discretion in emergencies, but her advice is always resisted. The story opens on the mother, aged 43, who has brought up a son and daughter to maturity and now wants to revert to her medical studies, being discontented with the social and domestic duties she has done so well, and not wishing to be merely a helper to her husband but to have a long-postponed career. She takes up her studies, but finds her mind too stiff as compared with that of her son, and in the sequel, after long delay and a good many episodes, reverts to being a wife and mother. The grandmother, 63, is a no less interesting study. She never had culture aspirations but was a good although bigoted family mother till her husband died and her children married, when she felt aimless, set aside, and finally drifted to psychoanalysis, which has been a fad of both her children and grandchildren, and despite many shocks became devoted to it and was given a new life by it. She collapsed, however, when she could no longer afford her bi-weekly seances with the specialist to whom it was a great satisfaction to her to pour out her soul. The granddaughter painted, wrote poetry, abhorred everything Victorian, faced even the most delicate questions openly and with an almost stunning frankness, and fell in love with a fine young man whom she had won away from her older aunt. She had free-love ideas and waged a very long and persistent argument with the lover against being tied by the marriage bond, yielding to his old-fashioned scruples only when she found she was otherwise certain to lose him. Perhaps most interest centers in the aunt, Nan, a successful novel writer, wonderful swimmer, bicycler, ultra-emancipated hyper-Freudian.

The moral of the book is that the four principal characters illustrate four very critical stages from which all, after a more or less prolonged period of circummutation, emerge into sanity. The style of the book and the author's interpretation of psychoanalysis are remarkable.

Dream Psychology. By Sigmund Freud. N. Y., McCann, 1921. Pp. 237.

The present reviewer of this work finds himself baffled to know who made the book. Freud has so lately published his "General Introduction to Psychoanalysis" that it seems hardly likely he would so soon write another work like this. Morever, the announcement of the publisher, J. A. McCann, says, "Here is presented to the reading public the gist of Freud's psychology in the master's own words and in a form which shall neither discourage beginners nor appear too elementary to those who are somewhat advanced in psychoanalytic study." In Tridon's colorless and ineffective introduction one finds no light shed upon the subject. The book is certainly not made up warp and woof of quotations, and what the reader will want the publisher to explain is what is meant by the phrase "in Freud's own words." How much, if anything, did Freud have to do with this, and who is responsible for its publication? It is by no means without its use and merits, but it does not strike the present reviewer as being the kind of digest Freud would have made himself, and he thinks that the reader should know Freud's real relation to the work.

The chapter headings are: Dreams have a meaning, The dream mechanism, The dream disguises, Desires, Dream analysis, Sex in dreams, The wish in dreams, The function of the dream, The primary and secondary process—regression, The unconscious and conscious—reality.

Man's Unconscious Spirit. The Psychoanalysis of Spiritism. By WILFRID LAY. N. Y., Dodd, Mead, 1921. Pp. 337.

This is the fourth book of the author on psychoanalysis, and here his thesis is that psychical research is on the wrong track. Mediums refuse to be analyzed "because unconsciously aware of the unconscious deception that they innocently practise." All so-called messages are really from the medium's own unconscious storehouse of memory images. So far as science knows, spirit is nothing. "There is no such thing to be revealed as a force operating from without upon real things with anything more like human intelligence than the swelling of water before it becomes ice." There are no breaks in the universe. "There is a wild attempt to guess out what will please the hearer without any attempt whatever to gain true breadth of wisdom and reality of thought." "Attempts to gain the *imprimatur* of science for the unconscious utterings of second rate minds have resulted only in the impartial and broadminded observer being repelled," etc. Evil messages are the pitiful dejecta of the unconscious of certain individuals. Instead of trying to prove spirits, men should recognize that this effort is only infantile. "The wish for proof is the direct result of the fear of death."

From this point of view all the eleven chapters are written. They are on the stream of consciousness, emotions, psychoanalysis, the unconscious as an urge, mechanism, unconscious emotions and the will, belief before knowledge, knowledge above belief, man's unconscious spirit, scientific investigation, the present status. The author's main thesis is only what almost every really scientific psychologist has long held, and is substantially that set forth by the writer of this note thirty years ago in the early volumes of this *Journal*. There are a number of striking new illustrations in the book, and it is easy and attractive reading, but it contributes little that is new to those familiar with psychoanalysis.

Getting What We Want. By DAVID O. EDSON. N. Y., Harper, 1921. Pp. 287.

The twenty-one chapters of this book might have been written as syndicate press-articles, for the author's sprightly style suggests Frank Crane. The sub-title of the book is: How to apply psychoanalysis to your own problems; but there is no wearisome reiteration of familiar Freudian nomenclature, and no effort to bring the interesting cases outlined under the classical rubrics of the analysts. The author's theories are strongly tinctured with anthropology, and the contrast between the dark and perhaps hook-nosed thinker and the blond blue-eyed doer is constantly in evidence. Indeed, these differences seem fundamental in the writer's diagnosis and therapy. There are four stages of human development; the archaic, dominated by hunger and love; the auto-erotic, which began with higher apes; the Narcissistic; and finally the social. Everybody wants to be great, but success in life largely depends upon determining the proportion of blond and brunet components in our make-up and regulating life accordingly. It is impossible to epitomize such a book. The analyst's work largely consists in seeing to it that men do not try to plow with a limousine or go on a picnic with a high-powered tractor. The man who inherited great wealth and came to the doctor to be cured of drowsiness is a typical case of the misfit in life of a psychic mechanism. In him every wish had been gratified except the wish to be great and to do something himself, and to this the routine of office life which he had to keep up gave no vent. He therefore took refuge in daydreams full of achievement, and in the summer vacation, when the archaic instinct of the old Vikings in him found expression in his yacht, he never complained of the drowsy feeling.

The *pièce de resistance* in the belated July number of *The International Journal of Psychoanalysis* is an autobiography, with comments by Dr. C. M. Haviland, written by a young man of twenty-three in one of the United States training camps on the eve of his departure for the battle front of France, compiled as a gift to a friend in case the writer failed to return. It breaks off abruptly, apparently in his eighth year, and his intention to bring it up to date in France was always frustrated. He wrote it, or rather he says it wrote itself, under the greatest emotional stress.

One of his earliest and most persistent concepts, when he was about three, was that he was of immense size and lived and moved inside a correspondingly large crystal sphere. He himself was an occultist, and believed this to be a reminiscence of a previous state of existence. Haviland, however, interprets it as a memory of prenatal life in the sphere of the uterus. It is an image "coming from the unconscious and thrown upon the screen of consciousness." To this sphere he was prone to retreat, for here he was absolutely monarch, and apparently he had not outgrown, even at the time of writing, a very vivid memory of it. At the age of four he had a distinct sense of the presence of another child, whom he often imaged as a white-robed guide, who at first directed him entirely in one of his most elaborate structures, *viz.*, the building of a sewer in his imaginary kingdom, later was consulted about everything, and finally slowly faded with years. This presence Haviland interprets as at first a mother-image, which had to be made masculine, and finally became identified with self. The child also developed an apparently purely original language in which he communed with the mysterious presence and also with the many two-inch people with whom he populated a wonderful fairy kingdom. His addiction to this language caused, for a time, the suspicion on the part of his parents that he was abnormal. The fairy kingdom was developed in great detail and had its own king, who was killed by the Crown Prince, himself later conquered and imprisoned in a high tower by an army led by an obscure boy within the province. The queen, whose features and traits were very vividly imaged, enlisted his deep sympathy. The countess, who was represented by a repulsive toad and was the chief mourner, dived from the royal barge and escaped death by swimming under water. This Haviland, we think rather lamely, interprets as a manifestation of the Oedipus complex. He makes no attempt to explain the very elaborate grave-yard in which all dead or dying animals were interred and where even insects were buried *en masse*; nor the persistent propensity to model catlike forms, a girl's pet and expressing his feministic tendencies; nor does he explain the persistent horror of all large animals and even of the barn in which they were kept. We should have welcomed some further statement as to the present state of this most interesting patient, whose childish imagination was so extraordinarily creative, illustrating spontaneous autistic powers quite as remarkable as those of Una Mary, Bashkirtseff, Mary MacLane, George Sand, etc. Analysts tend to explain such phenomena as due to an impulse of retreat toward infantile and even prenatal conditions.

Outwitting Our Nerves: A Primer of Psychotherapy. By Josephine A. Jackson and Helen M. Salisbury. N. Y., The Century Co., 1921. Pp. 403.

To one who is often asked to suggest a lucid and sprightly introduction to psychoanalysis this book is a godsend, for it best serves its purpose, as indeed, since it is the last of a long series, it ought to. The authors have had a long experience in dealing with diseases where there is "nothing to be cut out and nothing to give medicine for." And yet these troubles seem to be more common in the world today than those which are helped by surgery or drugs. In the sixteen chapters here we have not only a clear statement of principles but also many well-chosen illustrative cases and a brief epitome of the theories, methods, and results of psychoanalysis, with hints at its larger culture-significance and the latest contributions to the subject. The literary quality of this book should make it very popular.

REVIEWS OF BOOKS

Grundzüge der Reproduktions-Psychologie. By BENNO ERDMANN. Berlin und Leipsig, Vereinigung Wissenschaftlicher Verleger, Walter De Gruyter & Co. 1920. Pp. viii, 186.

To those who are acquainted with Erdmann's contributions to psychological literature, as well as to those who have attended his lectures on psychology, the main argument of the *Grundzüge* will not be new. The book presents a careful restatement and logical completion of his scattered discussions of a fundamental psychological process. The original discussions appeared in his numerous papers on thought and language, in his logical studies, and in his *Theorie der Apperzeption.*

To those who believe in the essential soundness of his account of the reproductive processes in perception and in thought, this restatement will be a welcome and convenient document. But the book was written for those who are not familiar with his doctrine. It was obviously motived by the desire to present a fundamental portion of his logical and psychological contributions in a form that would insure it permanence and a fair hearing. Erdmann makes very clear how much importance he attaches to it.

The book was obviously produced under difficulties. Controversial matter, originally planned for, was cut out bodily. The rest bears marks of condensation. The publisher is formally thanked for producing the book in spite of the "unfavorable times."

Notwithstanding its comparative brevity, the *Grundzüge* contains a careful presentation of the personal and historical conditions of its argument. A frank statement of epistemological and psychological postulates, defence of "arm-chair introspection," and a remarkable attempt to represent his underlying personal equation, are added to the more commonly expected acknowledgments to his teachers and associates, to his historical antecedents and to experimental data. The effort to give the reader all the information necessary for discounting the bias of the author, as well as for a critical estimate of the mental antecedents and personality behind the doctrine, is unparalleled in psychological literature.

If one sought the most fundamental principle of the *Grundzüge* it would probably be found in the doctrine of non-independent reproduction by apperceptive fusion. This is an immediate, necessary, and universal process in every perception, and consists of the arousal of certain representables and their fusion with the direct effects of stimulation. It should not be confounded with the associative fusion of sensory data into wholes possessing qualities, or with the associative interweaving of percepts by preestablished neural paths, by personal experience, or by similarity. It is the precondition of them all.

Any *Reproduktions-Psychologie* must start with the postulate of psychophysical residua. Their nature still remains a matter of debate. That they exist as unconscious dispositions to new moments of consciousness, that they retain as unconscious dispositions the associative interweaving of the original experience, there is abundant evidence in habit, both motor and perceptual, intellectual and emotional. The possibility of reproduction which is not conditioned by preformed paths or by experience; whether actual contents of consciousness are the exclusive conditions of reproduc-

tion; the possibility of reproduction which does not appear as conscious content; these, together with the function of attention in reproduction, are the main problems of the *Grundzüge*.

Sense-perception regularly involves memory-factors for which there is no direct sensory stimulus. These are commonly associated supplements. The consequent perception may be called associatively supplemented (*ergänzt*) perception. A still simpler and more fundamental supplemented perception is found in tachistoscopic experiment and occasionally in daily life. It occurs when attention is concentrated on the perceptual content, and also when objects are casually noticed in cases of diffused attention. In spite of the narrowness of this kind of perception, it is often clearer than the present stimuli can account for, and it commonly involves an identifying cognition. Either fact would imply the interaction of reproduced sensory experience. The notable peculiarity of such reproductions is that they never appear in consciousness independently, but always fused with the immediate results of stimulation. The term apperceptive fusion, which may be applied to such reproduction, must not be confounded with associative fusions of conscious factors. It refers not to conscious contents but to the conditions of consciousness. Apperceptive fusion involves two moments which may be called respectively the stimulus-component and the residual component. In any given fusion the two components are simultaneous. Dynamically, the stimulus-component is primary. The residual component, however, is responsible for the fusion.

Apperceptive fusion is the condition of all cognition. It determines the course of attention and is the cause of the illusions of normal and abnormal life. All cognition is recognition. No perception (even the most undeveloped) is entirely free from apperceptive fusion. In adult consciousness it underlies the serial development of observation, introspective as well as sensory.

The reproductive processes that begin in apperceptive fusion commonly lead to mediate supplemental interwoven reproductions. Of these, remembered, abstract, and imagined presentations are the simplest forms. Such reproduction is associative, but not in the sense of Hume's association of ideas. Only residua are associated with the immediately aroused component of apperceptive fusion through which they are reproduced. In mediate supplemental interwoven reproductions neither the associated residua nor the condition of their reproduction is a conscious content.

Of the various forms of supplemental interwoven reproduction the most momentous is the perception of symbols. The cognition of symbols presents every form of sensory cognition from apperceptive fusion to the more complicated thought-processes. In the discussion of these supplemental reproductive interweavings, Erdmann restates, partly in the form of equations, his contributions to the interrelation of thought and speech, as well as to the psychological organization that underlies formulated thought. The argument is too condensed for recapitulation.

Not only may reproducing moments be non-independently aroused residua, now fused with sensory moments, and again interwoven as associated supplemental moments, but also the products of reproduction may remain unconscious though stimulated. This occurs in apperceptive preparation for (or in attention to) a coming unknown stimulus, in the silent elaboration of speech, and, as Erdmann's self-observation indicates, in the lack of meaning-consciousness antecedent to familiar utterance. The understanding of sense impressions, of speech, and of reading matter may on occasion involve wide-spread conscious reproduction of agglutinated residua. When the material is sufficiently familiar, the stimulated agglutinated residua may remain unconscious. These unconsciously stimulated residua may be represented in consciousness by emotional states, of which the feeling of familiarity is an example.

The climax of apperceptive completion appears in the sublogical processes such as abstraction, comparison, expectation, and combination, which are the psychological foundation of formulated thinking both inductive and deductive.

Erdmann regards attention as a ground-function of the mental life. Whether it takes the form of clearness of the changing content of consciousness or the form of expectation, it involves a reproductive process which is determined by the connection of residua. In expectation that which is expected is commonly not given as an object of consciousness, though under favorable circumstances it may be. In the former case what is expected must be regarded as an excited representable, a kind of preconsciousness.

It is always a dubious process to present discontinuous bits of a careful discussion for approval or disapproval. In the present instance it were worse than useless unless it induces the reader to explore for himself the original mine of fine observation and far-reaching analysis.

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The Origin and Development of the Nervous System from a Physiological Viewpoint. By C. M. Child. University of Chicago Press, Chicago, Illinois. 1921. Pp. xvii., 296.

In this book the origin and development of the nervous system are considered from a physiological point of view.

The first five chapters form an introduction to the primary thesis, and are devoted to a discussion of pattern in the organism and the possible relation of pattern to the physiological gradients in general.

Protoplasmic pattern and organismic pattern, in the author's opinion, differ probably only in the order of magnitude; and the question immediately arises whether organismic pattern (the relation of parts in the organism) is inherent in protoplasm, and develops spontaneously, or is, in some sense, a response to environment. The subject-matter of the book is concerned with an attempt to answer this question. Excluding the purely contactual or mechanical factor, there are two chief categories of relation between protoplasm and its environment, the material or chemical and the dynamic or excitatory; and the latter—the excitation-transmission relation—the author believes to be the important factor in impressing the organismic pattern on the protoplasmic substratum.

He discusses the physiological basis of various pattern-types in plants and animals, attaching great importance to the physiological gradients (quantitative gradations in functional activity) in pattern-production.

The nervous system, *the* organ of integration, in its origin and development, does not involve the appearance of a new functional activity different from the fundamental activities of protoplasm in general; some kind of physiological continuity exists between excitation and conduction in protoplasm generally, and the development of the nervous system. "Living protoplasm is functioning at all times and development is a process of functional construction, that is, beginning with a given structure and function, the continuance of function modifies the structural substratum, and this in turn modifies further function, and so on." According to this conception, the author believes that the nervous system is the physiological and morphological expression of the excitation-transmission relations, first with respect to the primary or chief physiological gradients, and later with respect to the progressive developmental complications as they arise.

The localization of the nervous system is an example of surface-interior pattern, and the general direction of growth and differentiation is down the physiological gradients, beginning at the anterior end and extending posteriorly.

To account for the origin and development of the neuron-pattern the author suggests that this may be determined by the electrical polarisation of the cell (neuroblast), which must lead to changes in its physiological condition and activity. In the stimulation of living protoplasm generally the primary change is probably electrical and this electrical polarization of a neuroblast may determine changes in its rate of metabolism and consequently in its physiological activity.

In a discussion of this question at the present stage of its development we are hampered by a lack of knowledge of the fundamental nature of physiological processes and of the ultimate structure of biological organisms. Although the mechanistic conception of life may not appeal to all, mainly on account of the paucity of facts which can be brought forward to support it, yet the conclusion of the neovitalist that the relation between pattern and process will never be understood is unjustifiable. There is little doubt that the two have a constant and necessary relationship, and for his attempt to elucidate the possible character of this relationship in the origin and development of the nervous system the author is to be commended.

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Fünf Reden von Ewald Hering. Herausgegeben von H. E. HERING. Mit einem Bildnis von EWALD HERING. Leipzig, W. Engelmann. 1921. Pp. 140. Price about 75 cents.

Dr. Hering has earned the grateful thanks of all experimental psychologists by this reprint of certain classical addresses delivered by his distinguished father. The addresses are: Ueber das Gedächtnis als eine allgemeine Funktion der organisierten Materie (Vienna, May 30, 1870); Ueber die spezifischen Energieen des Nervensystems (Prague, [1882] 1884); Zur Theorie der Vorgänge in der lebendigen Substanz (Prague, [Feb. 18,] 1888); Zur Theorie der Nerventätigkeit (Leipzig, [May 21, 1898] 1899); and the Antwortrede to the award of the Graefe-medal made at the 33d meeting of the Ophthalmological Society (Heidelberg, [Aug. 6,] 1906). The text has apparently been edited, so far as editing was needed; I notice the removal of a troublesome clause from the third address (p. 64). The book is printed in large and clear type on paper of a rather poor quality. The frontispiece portrait is excellent.

There are some curious omissions. There is no paged table of contents; the fifth address does not figure in the list on cover or title; and we are not told where the addresses were originally printed. The Open Court translation (1895) of the first two—those on Memory and on Specific Energies of the Nervous System—is similarly silent. The address on Memory appeared in the *Almanach der Wiener Akad. der Wissensch.*, *Jahrg.* 20; a second edition of the offprint was published in Vienna in 1876; and the address was included in W. Ostwald's *Klassiker der exakten Wissensch.* (no. 148) in 1905. The address on Specific Energies was given at Prague in 1882 (see p. 79 of the present reprint) when Hering assumed the rectorate of the university; it seems to have remained unpublished until printed (1884) in *Lotos, naturwiss. Zeitschrift hrsg. v. d. deutsch. naturwiss.-med. Verein f. Böhmen*, vol. v. The third address was also published in *Lotos*, vol. ix. The Leipzig lecture was issued in pamphlet-form by Veit & Comp., Leipzig. I do not know whether the Antwortrede has been published before; it is not contained in the report of the Heidelberg Congress in the *Arch. f. Augenkeilkunde*, 56, 1907, 93 ff.[1] It is, as the editor remarks in his preface, of especial interest, since Hering uses the occasion

[1]The volume is wrongly given as 55 in the *Zeits.* bibliography (xlvii., 1908, 362, no. 654) and in the *Psychol. Index* for 1906 (13, 1907, 41, no. 649).

to review in brief the whole course of his scientific work, and to acknowledge his intellectual debt to Lamarck and Darwin, Schopenhauer and Fechner. We should all have connected him with three of these men, but I doubt if we should readily have thought of the fourth.

E.B.T.

The Physical Growth of Children from Birth to Maturity. By BIRD T. BALDWIN. University of Iowa Studies in Child Welfare, from the Iowa Child Welfare Research Station. Iowa City, University of Iowa, 1921, Vol. I, No. 1. pp. 411.

The author presents a comprehensive survey of studies which treat of physical growth, together with a more intensive study of the growth of (sometimes only a few) individuals taken at intervals over a considerable period of time. It represents a serious, methodical effort to establish norms which shall be based upon the growth of the same persons and not, as is the rule, upon different persons at different ages. Not the least important part of the *Study* is the plea for standard apparatus and for uniform procedure in making measurements. In the case of chart LIII, p. 150, where he gives the weight in pounds, the author departs from his demand for the universal use of the metric system for scientific work. One may or may not agree in the matter of choice of measurements to be taken; but one must admit that the 23 measurements chosen are, perhaps, as important as any others. We especially welcome the inclusion of psychophysical measures. Some might wish to take fewer measurements, and to work intensively till reliable norms are to be established,—thereby denying or ignoring the doctrine set out in the *Study*, which assumes that normal growth is not only a matter of amount but also of relation; that the relation of weight to height is to be considered just as well as weight itself. In any event, the author does well to emphasize the need of case-histories in an endeavor to determine the relation between growth and nutrition, disease, sex, race, geography and environment.

The first section of the *Study*, which deals with instruments and methods of measurement, is well illustrated by photographic reproductions, so that the form of the apparatus and the manner of their use are perfectly clear. Even more photographs would serve the purpose of standardisation, although the description may be taken as ample in most cases. Yet on p. 21 we find that "the child's *left* middle finger touches a vertical wall or moulding" and "that the observer applies the square lightly against the free end of the middle finger of the *left* hand:" a statement which is obviously at fault, although one which anybody with sense can correctly interpret; it only emphasises the importance of illustrations, since standardised procedure is, of all things, the prime requisite. Only a portion of the 23 measurements outlined find a place in most of the tables and charts; only 2 in the charts and tables for babies; 15 in the tables and 3 in the charts for pre-school children; 8 in the charts and 15 in the tables and profiles for school children, while an additional measurement comes into the chapter on anatomical age.

Chapters III, IV and V are replete with tables of averages for groups and of individual measurements, which show the absolute and relative gain in the various physical dimensions for various ages; with charts which give the growth-curves of individual boys or girls in a single dimension; and, of especial note, with the synoptic profiles of growth, which sum up in concise form the growth-history of the individual in 15 dimensions,—although the numerous inversions are inexplicable in terms of any tables given and the author says nothing about them. In the summary at the end of the chapter on infants there are two conclusions which seem to oppose each other. We are told that there is no positive correlation between weight at birth and weight at the end of the first year; in the following paragraph we read that from 60 to 70 percent of the babies which are above average

weight at the beginning of the year are above average at the end of the year and conversely. No figures are given; but it seems that, if 65 percent of the heavy remain heavy and 65 percent of the light remain light, a positive correlation of some degree would surely follow.

All this tends to emphasize the basic notion of the work, namely, the idea that study should be made of the same individual throughout the period of his or her physical development; that we should study the growth of individuals and not of masses. It results that there is no true single norm to which all should conform, but rather that there are general types or forms of curves of growth, and that the individual should approach to type although his curve of growth may rise above or fall below the average. By the time a child has reached school age, if there is a record of his growth taken at semi-annual or even at annual intervals, the course of the curve of growth has gone far enough for a forecast of future normal development.

Although growth should and does approach a norm, for after all children are more alike than different in their development, the notion that growth is an individual matter and a matter of interrelated parts with emphasis on the interrelations finds further amplification in Chapter VI, where we have the inter- and partial coefficients of correlation between 8 traits, 3 of which are psychophysical in nature; the coefficient of variation; the indices of growth, and the percent of increase from year to year. These, especially the indices of growth to which attaches a good deal of importance, help to give a more adequate picture of the total aspect of growth in all its parts and in all its relations. The figures of Table XXVII, p. 146, do not bear out the second conclusion on p. 147, "that for weight girls are more variable than boys at six years of age and six years later."

In Part III we find an unusual distinction made between anatomical and physiological age. The former finds its principal measure in the surface areas of the carpal bones as shown in roentgenograms and as measured by the planimeter. The reader need scarcely be reminded of the author's long and persistent interest in the subject of physiological age in its relation to mental maturity; yet this topic receives, perhaps, the least adequate treatment of any in the book. The sole criterion offered or discussed of physiological age is the age of pubescence for boys and of physiological maturation for girls, from which the author concludes "that girls who mature early are on the average close to the norm or below it," a fact not confirmed by the figures given on pp. 191-192, where we find that all girls who matured at the early ages were well above the average height of the groups of which they formed a part. It may be that the author means that the girls who mature at 11 and 12 years old are slightly below the normal height at the median age of maturation, which age is about 13 years 8 months.

The classification of practically all extant studies on physical growth under 10 main heads with some 60-odd sub-heads, with short paragraphs mixed in here and there pointing out trends, deficiencies and relationships, will form a logical starting-point, a point of orientation, for one about to start work along this line; it can do no more than orientate, but it can do that very well indeed.

The notes of reference, appended to the several extensive tables of Part V, which indicate the race or class of persons measured, may be a sufficient explanation of the wide variation in height found by workers in the same country (these differences amount in some cases to as much as 10 or 12 cm.); but when the difference in growth is as much as that indicated, it becomes all the more evident that not a single norm but a set of norms or limits of normality must be our guide, and that the ratios or indices of growth may be the most valuable measure.

The last, though not the least valuable part of the *Study*, is the annotated bibliography of 911 titles which makes up the bulk of the concluding section. We may say in summary that the *Study* is important, not for what

it completes (for the norms and conclusions will have to receive much verification or modification, since they are in many cases based on too few cases), but rather for the programme of work laid out. Because it is programmatic, rather than final, it holds all the more interest for any one who would carry forward the task set; the task of completely understanding physical growth in all its aspects, relations and dependences.

L. B. HOLLINGTON

Poetic Origins and the Ballad. By LOUISE POUND. New York, The Macmillan Company, 1921. Pp. x, 247.

From a study of early poetry and songs recorded in manuscript or passed on orally from generation to generation, the author brings together evidence to support the several theses which she maintains throughout the book. She insists that "there is no sufficient proof that narrative lyrics were ever, anywhere, at any time, by any people, made and sung at the dance." The dance songs of primitive peoples are not narrative, and the earliest English dance songs are not narrative. When "real ballads" are used as dance songs they tend to decay by repetition, and songs used as dance songs do not develop into ballads, but are simplified into some "striking line or formula." Her second contention is that the authorship of primitive poetry is not "communal," but that "the gift of song seems as instinctive in man as the gift of rhythmic motion, not a development from the latter. Children sing instinctively, and they make their own songs, without waiting for the communal inspiration of group dancing." Hence it is reasonable to assume that primitive verse-makers produced their own poetic constructions. The author believes, thirdly, that the ballad appears rather late in literary history, if by ballad we mean a song-tale. The fourth thesis is that "incremental repetition" is not peculiar to the ballad alone but appears in "all types of popular poetry, from nursery songs to revival hymns," and therefore may neither designate the ballad nor furnish evidence of its origin. It is maintained, fifthly, that the "story song is not a primary but a developed type in the evolution of literature," and emerged from a "higher origin than unlettered folk-improvisation." The type of song-tale created among modern cowboys, soldiers, negroes and other groups is relatively inferior; and yet these groups ought to be no less capable than primitive society of ballad production. Finally, the author urges that it is not true, as some assert, that no more ballads will ever be composed. Perhaps no more ballads of the Child type will be produced, but there will be ballads of new types.

In explanation of the mediaeval ballad literature the hypothesis is offered that it may have "emerged under the influence of the clericals, or in something like it."

The thorough manner in which Professor Pound has collected representatative primitive literature and her study of it give weight to her contentions, a support which they especially need since they are in opposition to the prevailing theories of the present time. Only the professional student of literature, however, is competent to render expert criticism of the book. The evolution of poetry no doubt throws light upon certain phases of what may be called mental phylogenesis, but it has been outside the author's province to develop her material from this point of view.

H. G. BISHOP

Grundriss der Psychophysik. By G. F. LIPPS. 3te, neubearbeitete Auflage mit 6 Zeichnungen. Berlin & Leipzig, Vereinigung Wissenschaftlicher Verleger. 1921. Pp. 132. 25c.

This little work appeared first in 1903 (167 pp.) as no. 98 of the Göschen collection. In its original form it gave a straightforward and unprefaced account of psychophysics as the border-discipline between psychology and

physics (in the widest sense), based upon an empirical principle of parallelism. The six chapters were entitled: Problem of Psychophysics, Foundations and Range of Psychophysical Parallelism, Qualitative and Quantitative Modes of Determination of Psychophysical Parallelism; Stimulus and Sensation, Feeling and Expression of Feeling, Subjective Perception and Objective Constitution of Spatial and Temporal Forms. The plan is apparent and very simple.

The new edition shows great changes. Chapter I, on the Foundations of Psychophysics, discusses the relation of special science and psychology to philosophy, mental development individual and social, mind, and the relation of mind to body. "The same things," we are told, "which are accepted by the special sciences as self-subsistent and as grounded in their objective existence, and which are regarded by psychology as manifestations of our own living existence, are considered by philosophy as the revelation of the endless, eternal life which underlies the whole of reality and our own self." A far cry from empirical parallelism—of which, by the way, there seems now to be no single word in the book. Mind is the operation (*Wirken*) that comes to consciousness in the exercise of our vital activities (*Lebensbetätigung*). Psychophysics is psychology, a "psychology directed toward the discussion of the connection with the fundamental (physical) determinations of the objectively subsistent."

This introductory chapter occupies 56 of the 132 pages. It is followed by a transitional chapter entitled Vital Condition and its Expressions (*Der Lebenszustand und seine Aeusserungen*). The simple reaction serves as text for sections on the instability of vital condition and on the adequate (mathematical) representation of its expressions. A third chapter (49 pp.) leads us into psychophysics proper, and covers roughly the same ground as the last 130 pp. of the first edition. The treatment of the senses has been greatly condensed; the methods have suffered less. The most important section, systematically, is §11, on *Ordnen und Messen* and *Die Bedingtheit des Wahrnehmens und Empfindens*.

Whether the new plan and the new matter are judged superior to the old will depend upon the reader's special interest. In one respect the present edition is definitely inferior to its original: the legible Roman type has been replaced by two sizes of Gothic, the smaller of which is trying to the eyes.

A Defence of Philosophic Doubt, Being an Essay on the Foundations of Belief. By A. J. Balfour. A New Edition. London, Hodder & Stoughton, Ltd.; New York, G. H. Doran Co. (1921.) Pp. x., 355. Price $5.00 net.

A reprint, with "some trifling verbal alterations and a few notes," of the essay of 1879, which has long been out of print.

An Outline of Abnormal Psychology. By James Winfred Bridges, Assistant Professor of Psychology, University of Toronto. Second edition revised. Columbus, Ohio, R. G. Adams & Co. 1921. Pp. 226.

This directive and mnemonic Outline appeared in 1919; the call for a second edition proves its usefulness. "Sections have been added on the subconscious, on hunger, on abnormal forms of religion, and on several other minor topics; a blank leaf has been inserted at the end of each chapter for annotations; and a subject index has been appended." The writer, we note, still uses 'hypothecate' in the sense of 'assume.'

Introduction à la psychologie: l'instinct et l'émotion. Par J. Larguier des Bancels. Paris, Payot et Cie., 1921. Pp. 286.

A series of essays, pleasantly written and full of historical information, leading up to the author's Theory of Emotion, already familiar to readers

of the *Archives de Psychologie*. The chapters are entitled: Aim and Methods of Psychology, Body and Mind, Consciousness and the Nervous System, Spinal Cord and Brain, Reflex and Cerebral Activity, Instinct and Emotion. The book will prove delightfully stimulating to the advanced student, but we can hardly regard it—at least for this country—as a useful introduction to psychology.

Readings in Philosophy. Compiled by A. E. AVEY. Columbus, Ohio, R. G. ADAMS and Co. 1921. Pp. xii., 683.

Quotations and references, intended for class-room use with Leighton's *Field of Philosophy*. The quotations cover a wide range, from the Old Testament and the early Greeks down to Russell and Royce.

PSYCHOLOGICAL PERIODICALS

Arch. f. d. ges. Psychologie, Bd. xli., Heft 1 u. 2. F. HERMANN. 'Der Einfluss des Kontrastes auf den Sukzessivvergleich innerhalb eines festen Reizsystems bei Augenmaesversuchen.' [Experiments on the successive comparison of horizontal lines, undertaken to determine the effect of the group, *i. e.*, of an obscure background-idea, upon the course of judgment; such effect should show itself mainly by way of contrast. The experiments fell into three groups, each one of which comprised a number (15, 9. 10) of complete series, with as many N, whose component stimuli were thrown together by chance. (1) Absolute impression, which already involves a consciousness of difference, bears more heavily upon the first than upon the second R shown; (2) along with the main tendency to form a mean-idea of the whole group goes a tendency to form partial means of (three) subgroups; and (3) upper deviations from the mean 'normal' idea produce a greater effect than lower deviations, while this idea itself lies lower than the arithmetical mean of the group.] A. MUELLER. 'Beiträge zum Problem der Referenzflächen des Himmels und der Gestirne.' [Reviews some twenty publications that have appeared since the issue of the author's book *Die Referenzflächen des Himmels und der Gestirne* (1918). Not much has been gained; the Göttingen dissertation (1919) of H. Stücklen, however, contains new and valuable ideas. We need further systematic observations, especially observations made on the ocean, and we need a more exact investigation of visual space: several special problems are outlined.] A. KIRSCHMANN. 'Der Metallglanz und die Farbe der Metalle.' [The author repeats his view that all lustre depends upon parallax: surface lustre upon binocular parallax and the parallax of movement, metallic lustre on the parallax of indirect vision. He seeks on this basis to answer the two questions why the strong body-colors of metals are always red or yellow, and not green, blue or violet, and why the strong red and yellow have but small coloring-power in alloys. Both of these facts are accounted for by the hypothesis that a metal is composed of highly transparent, probably crystalline 'particles,' with high index of refraction. The possibility of constructing pseudo-metals from thin sheets of mica confirms this theory.] R. BECK. 'Beruht die gegenwärtige Vorstellung des Hochgebirges als schön auf einer Aenderung der menschlichen Ideen von Schönheit?' [High places (Sinai, Olympus, Mount of Olives) were honored in antiquity; but the ancients made roads and journeys for commercial and military reasons, not for pleasure. It is social conditions, not human nature, that has changed.] F. GROSSART. 'Das tachistoskopische Verlesen unter besonderer Berücksichtigung des Einflusses von Gefühlen und der Frage des objektiven und subjektiven Typus.' [Experiments with the Wundt tachistoscope. (1) Factors at work during the original process of apperception are mood, familiarity, ideas in preparation, egocentric ideas, subjective form-quality; factors in subsequent readings are feeling, ideas conditioned

by reflection, auditory image, and wrong associations set up in the visual image by the readings. Feeling is of extreme importance for subjective conviction and for the contents of the *Aussage*. (2) For the objective evaluation of important *Aussagen* account must by all means be taken of type. The objective type is passively receptive; the subjective is actively elaborative. The subjective type has, however, two sub-forms, self-activity with criticism and self-activity without criticism; the former may be even more reliable than the objective type. In general, type is of more importance than practice.] G. E. MUELLER. 'Gesellschaft für experimentelle Psychologie.' [Notice of the 7th Congress, Marburg, April 20-23, 1921.]

Zeits. f. Psychologie. Bd. lxxxvii., Heft 5 u. 6. A. PRANDTL. 'Die psychische Leistungsfähigkeit bei wechselnder Disposition.' [Experiments methodically planned to bring out the difference of mental performance in health and in indisposition (consequent on rotatory vertigo) show that the essential condition is intensity of set (*Einstellung*), *i.e.*, of an organisation of the central processes which is directed on distribution of available energies in the sense of a determinate end (*Ziel*), and elimination of useless and actuation of useful reactions. The result throws critical light on studies of fatigue, and permits the 'feelings' of assurance, uncertainty, fatigue, etc., to come to their rights.—These experiments indicate that reproduction suffers but little, impression considerably, from indisposition. Further experiments prove that retentiveness or capacity of repetition also suffers, and thus raise the question whether retentiveness at large may not be more variable than, *e.g.*, James supposed.] H. H. Keller. 'Experimentelle Beiträge zur Lehre vom Wiedererkennen.' [(1) Experiments with mixed series show that, after a 10 min.-interval, nonsense-syllables are better recognized than monosyllables. After a 24-hr. interval this relation is reversed. (2) The former result indicates that the nonsense-syllables receive a *plus* of attention. Experiments with mixed series of ordinary nonsense-syllables and familiar nonsense-syllables remove this advantage, so that the familiar material now takes the lead in recognition after both intervals. (3) The second result suggests Jost's laws. It is found in fact that in the course of 24 hours a given degree of recognitive familiarity of syllables decreases more slowly if the familiarity is of old standing than if it is more recent (analogue of Jost's second law); and that renewed impression has, as regards later recognition, a greater value for the 'older' than for the 'younger' syllables (analogue of Jost's first law).] I. HERMANN. 'Ueber formale Wahltendenzen.' [Experiments with numerical intervals and spatial series, performed on children and adults, normal and insane subjects, groups and individuals, bring to light two tendencies of choice: the one to choose a limiting, the other to choose the middle member of the series. The former is the more primitive; it implies a set for the stimulus and economy of movement; the latter implies something like form-quality, with realisation of equilibrium, stability of the figure, symmetry.] Literaturbericht. Psychologische Gesellschaft zu Berlin. [A committee, consisting of R. Baerwald, M. Dessoir, A. Moll, has been appointed to investigate clairvoyance, telepathy, telekinesis, materialisation.]

NOTES

SYNAESTHESIA IN A CHILD OF THREE AND A HALF YEARS

Edgar Curtis is the son of Professor and Mrs. O. F. Curtis of Cornell University. At the time of this writing he is three years and seven months old. He has never been particularly interested in colors, and he knows only the names of hues of good chroma. He calls rose, and various tints of pink, red. He uses his own descriptive words, however, and he often calls a color reddish, red and orange, etc.

About two months ago his mother noticed for the first time that apparently he has colored hearing. Their home is not farfrom a rifle range, and the sound of the guns resounds through the hills with a loud 'boom'. One day Edgar asked: "What is that big, black noise?" A few days later he was being put to bed on the sleeping porch. Two crickets were chirping loudly, one of them having the usual cricket-sound with which he is familiar, the other having a very high, shrill chirp in comparison. He asked: "What is that little white noise?" When his mother told him that it was a cricket he was not satisfied, and he said: "Not the brown one, but the little white noise." Then he imitated both of them, calling the lower brown and the shriller of the two white. At another time, when a cricket-chirp uttered from farther away came with a resonant buzz, he called it red.

He calls the sound of the cicada white. The electric fan is orange, and the electric cleaner which has a deep 'burr' is black. The sound of a frog, neither very high nor very low, is bluish. A little Japanese bell is red when rung loudly, and white when it tinkles faintly. A squeaking door is black and white. One could distinguish in that sound two tones of different volume. Drumming on the back of a guitar, when the opening is held to his ear, is black. An engine makes a black noise, but an electric pump is black and white. The low notes of the chimes are brown and black. The shrill crying of a little child is white. The rhythmic rise and fall of the noise made by a street-car in motion is orange. A can is black when it is pounded upon, and when the sound is dulled by touching it with the finger it is red. Thunder is black. A Scotch woman with a broad burr in her speech read him a story, and later he said to his mother: "Do you know what color it is when she reads? It is black."

All the above information has come from the child's casual conversation. He takes it for granted that everyone has the colors that he has, and will often remark: "That noise is red, isn't it?" His parents have been careful not to suggest colors to him, and they have not either suggested that a sound may be of a different color from the one he has named. During a few little experiments, the experimenter sometimes said, "I think that color is white," when Edgar had said it was something else. Every time he was very positive that he was right, and he was manifestly disgusted that anyone could think the sound was white when he had said it was red. He often goes to the piano when he is alone in the room, and to amuse himself touches the keys and tells the colors of the sounds. Notes have been made on those colors when he was not aware that he was overheard. Middle-C is red, and the tones just below are red or red-purple. The bass is black, and the high tones are white. Between middle-C and the white tones are reddish and bluish tones. Edgar never of his own accord named tones yellow, green or gray; but during some later experiments he found tones for them after seeing the color. One day, upon seeing a rainbow, he

exclaimed, "A song, a song!" We thought that this reaction might be a mere matter of association; and we decided to see whether, if he were shown colors, he would find the corresponding tones on the piano.

Red, orange, yellow, green, blue and purple papers of good chroma were used, with the addition of black, white and middle gray. He played with the colors for a few minutes and he was delighted with the idea of trying to find them on the piano. Following are the tones he selected, every color having the tone named and one or two tones above or below.

a''' and all tones above	White
b''	Yellow
e''	Green
e'	Blue
c' (middle-C)	Red
a	Orange
A	Grey
E and all tones below	Black

He selected the tones by playing about on the keys with one finger, and saying, *e. g.*, "This isn't red! This isn't red!" and then gleefully, when he found a tone that suited him, he exclaimed: "This is red, isn't it?" It was interesting to notice that when he was searching for red he did not explore the white or black region, but when grey was given him he went immediately toward the black, and when yellow was given him he went toward the white tones.

We thought that tones of the same musical pitch might possibly be of the same color to him. We found, however, that on the guitar white was *e'*, which on the piano was blue. On the guitar, *c*-sharp was black, though that region on the piano was red and orange. On the guitar, again, *g*-sharp was red and black, while it was red on the piano. One high tone on the guitar was called "a little baby white one."

From Edgar's own adjectives, and from the distribution of the colors on the keyboard, it seems that noises or tones of low pitch and large volume are black or brown or grey, while shrill, high, piercing, thin tones are white; the other colors range over sounds of intermediate pitch and volume. The normal order appears to be orange or orange-red, red, red-purple, blue; then follow, under the experimental conditions, green and yellow. There is some uncertainty as to the red-purples. Our investigation has, however, been so imperfect that such uncertainties were to be expected; it is only the primary and general outcome that we wish to emphasize. We hope that later studies may be made under stricter experimental safe-guards.

Anna Kellman Whitchurch

Cornell University

URBAN'S TABLES AGAIN

Typography of statistical tables is so difficult that complete accuracy is long in being achieved. Urban first published his tables for the method of constant stimuli in 1912 [*Arch. f. d. ges. Psychol.*, 1912, 24, 240 f.]. He reprinted them, making two corrections that he had discovered, in the *Praxis der Konstanzmethode*, 1912, 20 f. Then Rich [*Amer. J. Psychol.*, 1918, 29, 121] discovered a third error, and Godfrey Thomson reprinted the tables with all three errors corrected [W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, 1921, 194f.] Now Mr. Howard H. Long of Paine College discovers that the value of $2\gamma P$ for $p = +.73$ should be 0.7551 (exactly 0.755068), instead of 0.7541 as it occurs in all the printed tables. Apparently a typographical error!

This discovery of the fourth error affects Rich's checking tables [*Amer. J. Psychol.*, 1918, 29, 120 f.; Brown and Thomson, *op. cit.*, 198 ff.], which have taken account of the first three corrections. In Rich's tables the value for $p = .73$ and $x = +2$ should be 7.2317 (instead of 7.2307), and the value for $p = .73$ and $x = -2$, 2.2365 (instead of 2.2373).

It may be well to suggest, in case someone again reprints the Urban tables as now corrected by Mr. Long, that the tables should be set up with horizontal rulings every five lines. This change Urban himself recommended after the issue of the *Praxis*. Both the tables in the *Praxis* and in Brown and Thomson have to be ruled by pen if they are to be put to much practical service.

E.G.B.

EXPERIMENTAL PSYCHOLOGY IN THE TALMUD

The following tale from the Babylonian Talmud[1] shows (if it be true!) that the method of expression and the use of the plethysmograph would have been understood by a psychological audience of the first century of our era. The tale is told of Vespasian, who is conversing with Rabbi Johanan ben Zakkai.

"In the meantime there came to him a messenger from Rome and spake to him saying: Up, for the Caesar is dead, and the nobles of Rome have agreed together to choose thee for their Caesar. Now he had at that time drawn on one of his shoes, and was in act to draw on the other, yet would not his foot pass therein; then he set himself to pull off that which he had drawn on, but it clave to his foot. Thereupon spake Johanan to him and said: Be not disquieted! Thou hast received good tidings, and it is written: A good report maketh the bones fat.[2]—What, then, said he, must I do?—And he said: Make to pass before thee some man that thou hatest; for it is written: A broken spirit drieth the bones.[3]—Then he did as Johanan had counselled, and his foot passed into the shoe."

Cornell University S. Feldman

THE MAX KLINGER BUST OF WUNDT

Dr. Walter N. Niles, of the Carnegie Nutrition Laboratory, has been good enough to inform me that a photographic reproduction of the Klinger bust, to which I referred in this Journal, xxxii., 1921, 177, is issued as no. 225 of the postcard series of the Museum der bildenden Künste zu Leipzig (Verlag von E. A. Seemann in Leipzig). The pose is that of the lecturing Wundt: the head is thrown back rigidly, and the moustache is lifted above the lower lip, in a way characteristic of Wundt on the platform but unnatural to him in any other situation. The whole head is magnificently conceived, and the longer one lives with it the more certainly can one read parts and phases of the familiar Wundt out of it. It is, however, not a portrait bust.

E.B.T.

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[1] *Gittin*, 56 *b*; L. Goldschmidt, *Der babylonische Talmud*, v, 1912, 546. This redaction of the Talmud was completed c. A. D. 500.

[2] *Proverbs*, xv, 30.

[3] *Ibid.*, xvii, 22. These two sayings, which themselves contain the gist of the whole matter, belong to the earliest collection of Proverbs, the accepted date of which is not later than B. C. 250.

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AN EXPERIMENTAL STUDY OF CERTAIN INITIAL PHASES OF THE PROCESS OF ABSTRACTION[1]

By HORACE BIDWELL ENGLISH

INTRODUCTION

In all previous investigations of the process of abstraction, the subjects were instructed, generally most explicitly, what to abstract and what to abstract from. Thus Külpe (10) in the earliest study instructed them to attend to one quality only, such as the color, of a complex object; while Fisher (7) in the latest and most complete investigation instructed her subjects to define certain groups—a task which to civilized adults could mean only to abstract the common element. Such procedure obviously leaves uninvestigated the impulse which leads one, without such instructions, to react to a situation by making an abstraction. Is it true, as Max Müller says and as Wundt implies, that abstraction springs from our weakness rather than our strength; that we abstract because we are unable to have regard to the complex fullness of experience? Or must we in some form revive Hume's "distinction of reason?" This is the problem we set ourselves to study.

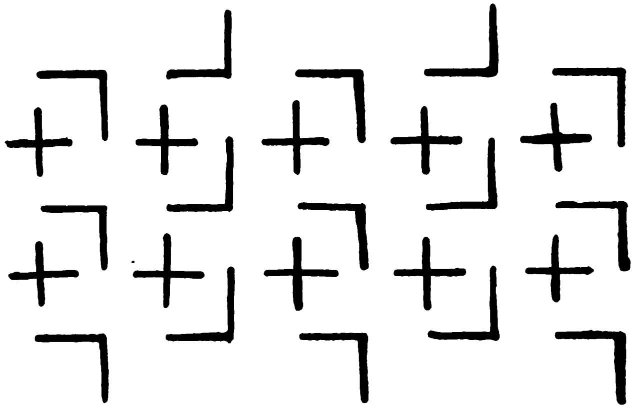
METHODS

The essence of the experimental method adopted was to put adult subjects in the presence of certain carefully-prepared situations and to require of them a report of their reactions. These were then studied for indications of the first steps toward abstraction. No attempt was made to separate two factors in-

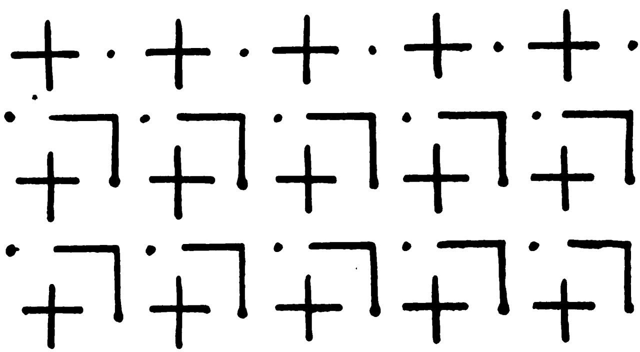
[1]A condensation of a dissertation accepted by the Faculty of the Graduate School of Yale University in partial fulfilment of the requirements for the degree of Doctor of Philosophy.

[illegible]

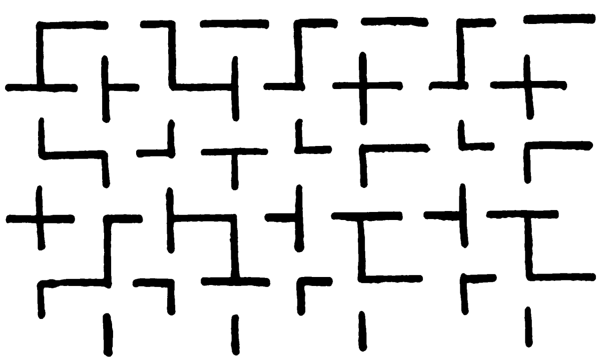
Each picture was shown for ten seconds, then after one minute, during which the subject read or conversed with the experimenter on indifferent subjects, again for five seconds. After two minutes, another picture was shown. A very simple tachistoscope was used. The instructions, which were read verbatim to the subjects each session until perfect familiarity rendered this unnecessary, were as follows:



LIVAB



CIPUJ



JEKOS

FIGURE I

"I shall show you a picture and a nonsense-word for a short time. Repeat the word aloud, once, while you regard attentively both word and picture above it."

Subjects were warned not to think of the experiment during the interval between pictures or between sittings.

For the first ten sittings, one each from the first seven sets of pictures was shown with its associated nonsense-word. In the second ten sittings from seven to nine pictures were shown. This included always one each from the last three sets of pictures and a selection from the seven sets learned in the first ten sittings. Certain pictures were shown more frequently than others. In some cases also, a picture was shown two or three times before a second member of the series was shown. This was done in an effort to bring out the effect of repetition and of dissociation by varying concomitants. Five minutes' rest was allowed after the presentation period. The subject was then placed in front of a simple exposure apparatus and the following instructions were read to him:

"You will now be shown the nonsense-words you saw with the pictures. As soon as the meaning of the word is apparent to you, react by pressing this key. You will be asked to report in what guise or form the meaning comes to you."[2]

The subjects demanded a definition of "meaning" and were told to give the word its everyday significance.

From time to time nonsense-words not in the series at all were introduced as "Vexierversuche" and in the second ten sittings nonsense-words belonging to pictures previously "learned" but not seen during the presentation period of that sitting were occasionally presented for reaction and report. It should be noted that one subject (An.) was familiar in a general way with the arrangement of the experiments; but full account of this fact will be taken in evaluating and classifying his protocols. Subjects wrote their own protocols and were questioned upon them very sparingly. Sittings were held three times a week and lasted about fifty minutes. The experiments of this group were held in the spring of 1915.

Group *b*

In place of the pictures shown in Group *a* musical selections were played on a phonograph, the nonsense-word being visually presented simultaneously. Allowing for the change of material, the same instructions were given as in Group *a*. The common element in each of the first four sets was the manner of producing the selection. In the first set, all the five selections were baritone solos; in the second set, duets by male and female voices; in the third, mixed vocal quartets; in the fourth, band selections. The fifth set introduced a cross-classification. All of the foregoing were small ten-cent records requiring less than half the time to play taken by the standard large-sized records.[3] The music was not only "raggy" but, except for the band selections, atrociously executed by both "artists" and record manufacturer. Set five consisted of two standard large-sized records, well executed. The music also was superior, even the popular "Tipperary"

[2]As the buzzing of the Ewald chronoscope used in timing these reactions disturbed and hurried some of the subjects, the stop-watch was later substituted. Even this might have been eliminated, since no use was made of the reaction-times. It is advisable, however, to have the subject react in some decided way in order to put a period to his train of thought; and when this is the case, the stop-watch may be harmlessly employed.

[3]Our subjects found great difficulty in remembering the selections well enough to form any constant associations at all; and, as the subjects were not allowed to see the records, the visual size of these records could not help them.

being better than the selections on the ten-cent records. Set five, therefore, was differentiated from the other sets by superior aesthetic qualities and by much greater length.

One subject could give but ten, the other but eighteen sittings to this part of the experiment, in which time no real abstractions were made. Certain preliminary stages of abstraction were, however, brought out. One subject (Av.) had read Aveling's book and supposed that we, like him, were interested in "imageless thought." His special knowledge of similar experiments probably played some part, but did not keep his protocols from presenting many points of interest.

Group *c*

Our experience in the experiments of Group *a* indicated two things. One was that the classification scheme should be more complex and more difficult. The second was that "nonsense-pictures" should be used, *i. e.*, pictures to which an English class-name cannot readily be given. It is impossible by this means wholly to avoid the influence of funded associations, as most investigators have found. It does not follow that there are not very great advantages in the use of nonsense-pictures. In nearly every case, it is true that the visual presentation will be apperceived as "like something" else and that the name of this "something else" will be used as a verbal tag to aid memory. But such a verbal tag will not suggest to the subject the basis of the grouping. An example will make the difference clear. Immediately upon seeing the first picture, *Hexur*, the subjects without exception thought "soldiers". Thus the basis of our grouping was given, and the subject's task was one of differentiation. This is an interesting process but not the one in which we are primarily interested. Upon seeing a certain *Jekog*, the subjects thought "dumbbell". This verbal tagging helped them to reinstate the whole picture, but it gave no clue to the one element referred to by *Jekog*, which was a certain pattern.

It proved to be by no means easy to construct a series of presentations that would answer our purpose. In the end it was decided to adopt the following scheme. A one-to-one relation between certain outlines (see Fig. II) and certain syllables, and between certain other syllables and certain patterns (see Fig. III), was determined upon. Each succeeding day a given outline would be shown, cut from a different pattern-paper, and every sixth day it would appear twice, *i. e.*, cut from two patterns. Similarly and as corollary, each pattern appeared with a different outline each day and with two every sixth day.[4] But whenever any pattern or any outline was shown, its syllable stood beneath it. Thus beneath each of these composite pictures was an outline-syllable and a pattern-syllable. Other features of the pictures such as color and orientation varied irregularly. The subject saw six pictures each day for two five-second intervals. Between exposures the subject read from a volume of letters. After going through the whole list twice in this fashion, the syllables were presented to the subject one by one and he was required to report his reaction to them.

The following instructions were read by the subject at every sitting until their content was perfectly familiar:

"I shall show for a brief exposure a picture with two nonsense-syllables under it. Regard the whole picture attentively and pronounce the nonsense-syllables aloud once."

"You will be shown one of the syllables presented in the exposition period just finished. When you feel that you know to what it refers, press down this key sharply so that I can hear it click. After a moment's pause

[4]The two presentations of the repeated pattern (or outline) were some distance apart in the series.

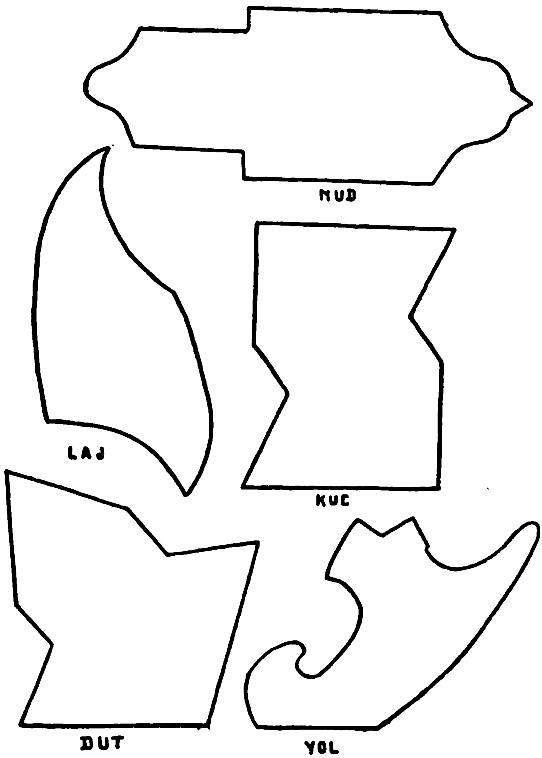


FIGURE II

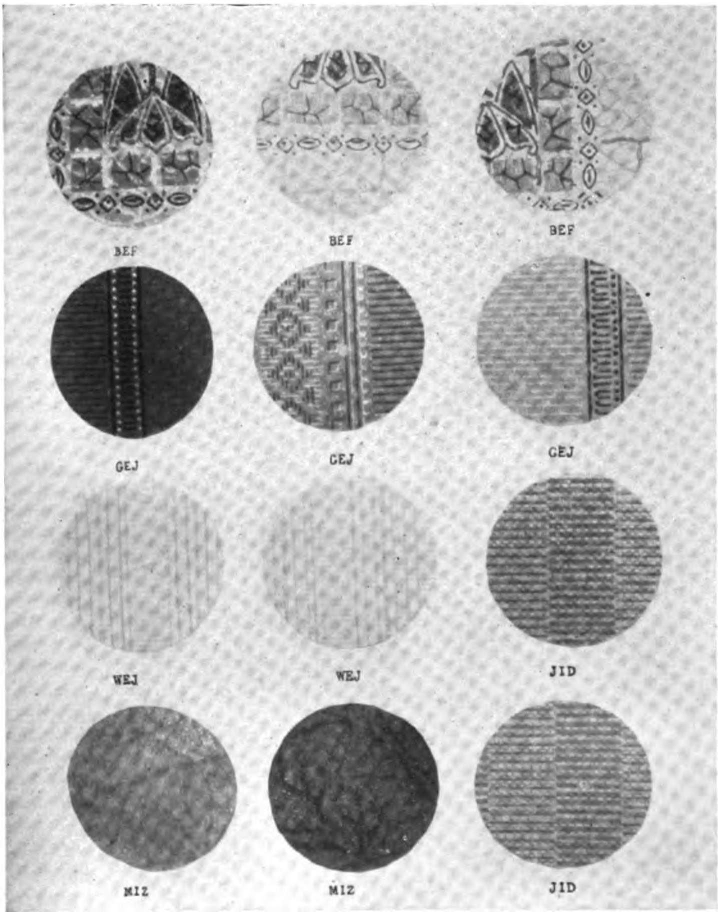


Figure III

in order to allow any further mental process to develop which may be relevant to the experience just finished, write an introspection of the experience."

The supplements to these instructions and the influence they had on the subjects will be considered later.

Beginning with the subject's third or fourth sitting, instead of writing his protocol, he was asked to dictate it to the dictaphone.[5]

One subject (S.) was completely inhibited when he tried to dictate an introspection to a machine. He simply could not utter a word and was obliged to write his report. Another subject (J.) was disturbed very considerably; after several day's trial it was decided that she should also be allowed to continue writing. The remaining subjects soon accustomed themselves to the procedure and very much preferred it. Where an experimenter does not know shorthand—as few do— the use of such a machine has very many advantages.

During the first seven sittings the pattern syllable was invariably given first and the orientation of the figures on the card was constant. In the seventh sitting and irregularly thereafter the two classes of syllables were in some cases reversed and the orientation of the two outlines *Yol* and *Dut* was radically altered. The intention was to dissociate these really variable elements from the two essential ones of pattern and outline. The actual effect was at first to draw attention to these variables and to set the subjects speculating on them. Finding no rhyme or reason in the changes, they accepted them as a fact and paid no further special attention to them. Three sittings a week were given during the fall semester of 1915. An. was too familiar with the problem to be wholly naive, while W. was undoubtedly somewhat influenced by her experience as subject in Group *a*.

Group *d*

This was a very short, control series in which the general plan of Group *c* was followed with auditory presentations substituted for visual. One set of syllables referred to the method of producing the sounds, another set referred to the tonal phrases produced. Three instruments were used: a zither (*Vop*), a small tubaphone (*Rev*), and a series of small organ pipes, (*Wug*). The phrases in terms of the key note were: Do-Do-Fa-Sol (*Kev*), Do-La-Mi-Fa (*Faf*), Ti-Sol-La-Sol (*Tuj*). Subjects were not allowed to see the instruments used. The instructions followed the lines of those in *c* slightly amplified, but two subjects (El.) and (Mp.) were told the purpose of the experiment. Another (Mn.) had been subject in *c*, but had not reached an abstraction, and was in the dark as to the purpose of the experiment.

It will be observed that the task set in Groups *c* and *d* is the same as in the latter part of *a*, that, namely, of abstracting from a series of situations a particular (as contrasted with a universal) common to them all.

In quoting from protocols, the initial of the subject is given first. The small letter indicates the experimental group, the Roman numeral indicates the number of the sitting within the group. The nonsense-word presented for reaction is next given, followed, in parentheses in the case of protocols from Groups *c* and *d*, by the syllables presented with it in the "presentation period". Thus P., *c* XII *Yol* (*Bef*) would mean that the quotation was taken from the protocol of Dr. Philp in sitting XII of Group *c*, dictated after reacting to *Yol*, one picture in that sitting having been combined from *Yol* and *Bef* (see Figs. II and III).

It should be noticed that our subjects were in no way required to give an analytic introspection of the process of ab-

[5]This was very kindly lent us by the Columbia Graphophone Co. We take this opportunity of acknowledging the courtesy.

straction. What they were required to do was to report the train of associations aroused by certain words. The nature of these associations, however, throws considerable light upon the progress and nature of the abstractive process. The task imposed did not prove a difficult one, even for inexperienced introspectors, requiring chiefly a fair psychological vocabulary. Nevertheless, we have nowhere based a conclusion upon the evidence of those subjects not accustomed to introspection, unless it is confirmed by one at least of the others. On the other hand, though we did not require it, a certain amount of analytic introspection was offered and is used as evidence.

Since our purpose is rather to discover and illustrate processes which are to be found in abstraction, than to determine those which are not, it is necessary to quote only so much from a given protocol as will clearly exemplify the point under discussion. Nevertheless, a great many protocols are quoted in full, especially where an effort is being made to characterise a certain phase or process as a whole. These reports are marked with an asterisk.

The Preliminary Phases as Analysis and Conception

We find that the process of abstraction begins in one of two ways: either the situation-complex is *assimilated as a whole* to some familiar conceptual category; or, where this is impossible, its unity is broken up and the situation is *analyzed* into such of its simpler components as will permit of such assimilation. Since a "real" analysis in things psychical has been denied (*e. g.* by Ameseder 2, 494 f.), it may be well to point out that by analysis we mean more than a distinction of the constituents of an experience. The constituent parts of an experience and the experience itself must be different after analysis. Now, as James says (9, 245), "the law is that all things fuse that can fuse." When, therefore, an originally unitary presentation not only is broken up into simpler components, but *remains* broken up, something must intervene to prevent an immediate recombination. For there would obviously be no inherent obstacle in the way of a fusion of elements which had once been fused.

And it is perfectly clear what this divisive influence is; it is the assimilation of the components to distinct mental categories. In analysis, at any rate as we shall use the term, the various features of the complex situation are thought of as relatively independent elements having validity outside the presented complex, as numerically distinct objects of thought. In James' terminology, they are conceived. If one cares to state it so, therefore, one may say that the first phase of the process of abstraction is the assimilation of the presentation either as a whole or in parts to preformed conceptual categories. Such a

statement emphasizes the essential likeness of the two types we have distinguished; it is more to our present purpose to bring out the differences between the first phase as analysis and as assimilation.

As was implied above, the chief determinant as to whether analysis or assimilation shall take place lies in the nature of the presented situation-complex. If this is some familiar object, assimilation promptly takes place. If, on the other hand, the situation as a whole is unfamiliar or novel, it is broken up, in a manner to be described, into such fragments as *are* familiar. Hence the presentations of the first ten sittings in Group *a* were nearly always assimilated, the new ones introduced in the second ten were more often analyzed. So, too, in Group *c*, most of the presentations were, from the first, subjected to analysis, but a few that looked most like familiar objects were assimilated.

For example, compare the protocols of subject W., taken after the first presentations of *Hexur* and *Jekog*.

"This gave me a visual verbal image of hussar. Then I saw immediately the three soldiers in the blue and buff uniforms. The image 'hussar' and the visual image of the picture seemed to be almost simultaneous."

"First I knew I had seen that. And I also knew I had only seen it once, and then I started to think about figures filled in with geometrical signs (visual imagery). I then got a visual image of the white hexagon filled in with 'pluses' and 'angles' done in red ink."

It should be understood that many of the protocols present odd intermediate forms or a mixture of both types. Analysis and assimilation are by no means exclusive. The essential difference between them is, of course, that, in the one case, the situation is treated piecemeal; in the other, as a whole. But it not infrequently happens that, an analysis having been performed, the subject discovers among the components one which somehow serves as a representative of the whole, and thus is led to the conceptual attitude.[6]

The Cues to Analysis

Taking up analysis first, our problem may be defined as the finding of the cues of analysis. Since our experiments were carried on with adult subjects, it may seem that we have in the analogy to past experience an easy solution. It is, indeed, too easy. Two forms of analogy may be meant. In one, the subject decides to analyze because he has previously found it profitable to analyze unfamiliar situations. Yet how he is to divide the

[6] "Attitude" is used throughout this paper somewhat loosely in accord with its untechnical but well-established significance, without regard to the question whether the "attitudes" correspond to the "Bewusstseinslagen" of Marbe (11) and Orth (13), or of the "fundamental personal attitudes of the self" of Calkins (6).

complex, to what previous situation the presentation is analogous, is still undetermined. We give some attention to these processes below under the heading of "Deliberate Analysis." Another form of analogy consists of a more detailed likening of the present to the past. Such analogy depends upon analysis far more truly than analysis depends upon it. It is only when we become conscious of the presence in the present situation of the same cues or factors which led to the analysis of the past situation, that past and present are thought of as analogous (cf. Bühler, S. 338, where such a "Regelbewusstsein" is said to be the basis of analogical thought.)

A second important point concerns itself with language. Although logicians and philologists had long ago decided that language is essentially abstract and analytical, no serious effort has been made by previous experimenters to determine the effect of the use of language upon the analyzing and abstracting consciousness. Yet one finds obvious traces of it at every turn. Thought, as Plato (4. 189 E) says, is little more than a conversation with oneself—at least, for most of us. And the necessity in these experiments of making a report, in words, forced even those not so much inclined to verbal thought to express in analytic terms what may have been a unitary experience. In no single instance in all of the protocols studied ([illegible] in all) was this factor entirely absent. But it works generally in close union with some other factor, so that the analyzing function of speech is dealt with chiefly under other headings.

[illegible] *Temporal Cues to Analysis.*—During even a five-second exposure attention does not remain upon any one feature of a presentation, nor is it of a constant level. A temporal or, what would seem to be equally effective, a clearness distinction of the components of the complex is thus effected. Exactly what it is that leads these temporally distinct objects to be conceived as numerically distinct is by no means easy to say, though we shall take up below some of the cues to conceptualization. We [illegible] at this point, however, upon the part played by language. Once separated in any fashion, the various features of the complex are almost invariably *named*. Now it is theoretically possible to use names and not think about the corresponding [illegible]. Yet the connection between a name and the [illegible] it signifies is so intimate, so frequently exercised, that [illegible] occurs without at least incipient general [illegible] accompanying it. Examples of this cue to analysis were very [illegible] and would undoubtedly have been more frequent had [illegible] on the presentation period.

[illegible] I noticed that their faces were different and I also [illegible] in which they both gaze, namely, from upper right to

J., *c* XVI, *Yol* (*Bef, Jid*). "I noticed in presentation period particularly its position with head lowered and with a red spot on its nose, as well as in central part of figure."

S., *c* XXI, *Nud* (*Wej*). "Visual image of [draws figure] with vertical lines across surface. There was a hesitation about reacting to this for a moment, but almost immediately the vertical lines leaped into greater prominence and the reaction proceeded."

Mn., *c* XIV, *Gej* (*Laj, Yol*). "I had two visual images upon the appearance of this syllable. The first one was the candle-flame picture, gray color, with the syllables *Gej, Laj* occurring beneath it. Then immediately it disappeared and I had the revolver picture, deep red color, the syllables *Gej, Yol* occurring beneath it. Then I reacted. I did not react after the first visual image because I felt certain that another visual image was coming, so I waited for it. In the case of the visual image of the candle flame it was the form, the outline, that was the most distinct and clear. I did not pay so much attention to any of the contents of the image. The form of the candle-flame picture was very bright, metallic, glittering around the edge, but in the case of the revolver picture I paid as much attention to the contents of the picture as I did to the form, I suppose because of the deep red color."

2. *Memory Cues to Analysis.*—The exposure of the syllable in the reaction period tends by a law of preferential revival to recall all of the situation with which the syllable was that day associated. But it only *tends* to do so. Some features are remembered quite badly, much is altogether forgotten. This is certainly due in very large part to differences in attention to these features during the presentation. Yet not altogether. Rather must we suppose that the same apperceptive interests (including the innate) which condition attention also favor recall.

In general, this distinction of elements becomes a conscious analysis in much the same way that the attentive distinction does. For several reasons, however, the transition is less abrupt. In the first place, the separation is much more complete. As has been above remarked and as will be illustrated below, some of the elements are entirely forgotten; the separation is here very nearly absolute.

A second and most important cause is the greater part played by speech. For example, after he had described rather minutely his visual images, S. wrote: "All of this was unitary; there was no temporal order in the visual process" (*c* VII). He is required, however, to characterize his visual image. Such characterization involves temporal separation of the various features; and *it involves giving to them some sort of name* in order that the experimenter may recognize them—by which means, however, the subject has for himself marked off the element from the rest of the world. This process may or may not occur where the selective function is attention; but, under the conditions of our experiment, this naming and describing of the features thus differentially preserved by memory is obligatory. The subject must tell *what* is vague, *what* clear. We might very

well put all such cases under the heading of "Analysis by Characterization." We prefer to keep these cases under the present heading on the grounds that the influence of memory is always clear, while the influence of the language factor is not always unambiguously indicated as important; and to separate cases otherwise so similar would be obviously artificial. This concrete difficulty in classification will serve, however, to illustrate the difficulty of drawing sharp lines of distinction.

The number of examples classed under this heading is very great, of which we can quote only a few.

An., *a* XIV, *Jekog*. "Then a blotch of yellow visually and then the yellow dumbbell with its inner pattern. Last not distinct; cannot reproduce pattern now."

C., *a* XII, *Livab*. "Auditory-motor verbal 'yellow' followed by a vague image of an irregular yellow figure with curved outline. Since writing, some vague visual imagery of figures composed of red lines."

W., *a* XVII, *Cipuj*. "I got a visual image of mustard-colored circle, but the figures inside were pretty dim."

Av., *b* VII, *Hexur*. "Word familiar, slight period of search; violin recalled, and a part of the melody very clearly."

An., *c* II, *Yol* (*Gej*). "A growingly distinct image of the fancy pistol pointed to the right, red, but with pattern indistinct."

Mc., *c* VII, *Dut* (*Miz*, *Wej*). "Not sure of color, but I have a good outline of the picture."

P., *c* V, *Yol* (*Miz*). "Also associated visually with a bright silvery picture, the general shape of which does not appear."

S., *c* IV, *Jid* (*Yol*). "With them came a visual image of a cream-colored brownish surface of vague outline."

W., *c* I, *Bef* (*Yol*). "It was not very clear; I could not remember the outline, but then red and green figures (square in form) stood out upon the tan background."

A definite sub-class is formed by those cases where the elements are distinguished not so much by inequality of retention and recall as by their time and space relations in memory. Three types occur: the one feature supplants the other; the one feature adds itself to the other and both persist as a composite picture: or the two are simultaneously present as visual images spatially separated. Owing to the independence of features thus appearing, these cases form very useful aids to conscious analysis. Both elements are present to consciousness, separately noted and (at least in the protocol, if not before) named; yet they are present not as perceived or imagined unities, but as distinct facts.

El., *c* IV, 6 *Gej* (*Laj*, *Kuc*). "Vest shaped figure, brown stripes. Followed by an appearance of the same figure with a green, slightly mottled surface, much more uniform."

W., *a* I, *Bekis*. "The word seemed familiar, and then immediately I said to myself 'first', meaning it was first in the series. I then got a visual image of the yellow drawing paper, as I had a decided feeling that the yellow drawing paper was used in the picture. Finally I remembered that I had first seen a circle in connection with this drawing paper. This was a visual image. The whole visual image was then very clear."

An., *c* III, 4 *Miz* (*Dut*). "Then the image of the same leafed pattern, with the tan hue, and immediately on that the form something like a zed."

Mn., X, 9 *Kuc* (*Miz*, *Wej*). "As soon as I had these two images in my mind, I could see them both, because each picture was of the same shape. It was a picture taller than it was wide, with an angle cut out of each side. One of them was brown and the other was gray. Under the brown one occurred the syllables *Miz-Kuc* and under the gray one occurred the syllables *Wej-Kuc*. Both images seemed to be perfectly clear in my mind. They seemed placed side by side with the syllables very distinct under them, both visual images."

How this subject avoided abstraction under these circumstances may well seem mysterious, but will be explained later.

3. *Associative Cues to Analysis.*—If we are reminded of one situation by another, it is in virtue of some feature or features identical in both. Not only in these experiments but also in ordinary life, the association is "immediate" or "unconscious", that is, the subject seldom or never reasons upon the logical implications of comparison just spoken of. Yet the presence in consciousness of a memory-image composed partly of elements found also in the presented object seems to favor the selective apperception of those elements of the presentation. Külpe (10) and Grünbaum (8) found somewhat similar results. Sometimes, of course, the favored features will be taken as representatives of the whole to which they belong, and in this case we have assimilation. More often the partial analysis begun thus by associative emphasis is continued in some other way. The number of examples of this cue is small in these experiments, but there seems reason to suppose it is to play a larger part under other conditions.

S., *b* III, 6 *Wocag*. "There was at the moment no reference to the music, but immediately after the reaction there came the meaning 'first exposure; music fairly like Old Black Joe'." The subject can only get hold of that part of the music-presentation which is like a familiar song. This phrase, however, the subject recognizes as merely one element in the song. He remains with what is felt to be an incomplete analysis. Subsequent presentations fail to enable him to complete it, and the subject's attitude gradually changes to the conceptualizing. The only meaningful phrase comes to symbolize the whole piece, and the subject refers to it as "that Old Black Joe plagiarism."

P., *c* XII, 6. "The syllable *Wej* is associated immediately with *Wej*, *Dut*, and almost simultaneously with the two pictures with the same-colored paper, one of them tall and thin and the other more bulky, smaller. In the images there seems quite distinct a tracing of the lines, especially the reddish lines. All lines are horizontal on the pictures, and *Wej* seemed to stand equally beneath either picture. No other verbal association came with *Wej*, *Dut*, which seems to stand underneath the first, the tall narrow picture, but as far as seeing in any sense the syllable *Wej*, it seems to be under both or under them as a single syllable. The pictures seem side by side and some way *Wej* in just one form stands under them."

4. *Analysis by Characterization.*—It is difficult to exaggerate the importance of our speech-habits in the process of analysis and abstraction. We have seen above that it preserves and con-

solidates the distinction effected by the selective agencies of attention and memory. It has, however, an independent analytic function. Our subjects were required to characterize their reactions to certain stimuli, chiefly in words, though often by drawings also. Now, both of these agencies are analytic. A visual image or a feeling or a concept may come to mind as a unitary experience. Anything approaching an adequate characterization requires an extended description by the use of spatially and temporally separated line-drawings or words. These lines, these words, have all been used by the subject in other contexts; and by whatever mechanism (whether subconscious "Bewusstheiten," marginal images, or what not) are present in this context as psychic universals, *i. e.*, as elements in other contexts as well. And it is precisely this process of breaking up a complex into elements having validity outside that complex which we call analysis.

We have spoken as if the necessity of characterization inevitably leads to analysis; and this is not, by our own showing, the case. The alternative is characterization which is incomplete, though adequate for particular purposes. Such characterization is dealt with later and need not detain us here. Whether in real life representation or complete analysis is more common, we cannot say. In these experiments, the comparative novelty of the experience led to more cases of analysis.

Examples, however, are hard to give. For while every quotation in this section and most in the next are couched in a form undeniably analytic, it is difficult to get examples to show that the analysis is *due* to the tools of characterization. Perhaps the clearest example is the following from subject S.:

S., *c* VII, 8 *Bef* (*Kuc*). "Verbal kinaesthetic processes followed promptly by visual image of creamy background with three green-red smaller figures. All of this was unitary; there was no temporal order in the visual process."

C., *a* XIX, 1 *Jekoq*. "Pronounced word to self. Period of search. Auditory-motor-verbal 'right angle' plus 'attitude'. I shall now proceed to analyze what this latter part means. It is 'plus' (auditory-motor verbal terms) and another 'attitude' which means this figure 1. I can't say whether this last comes as a visual image or not, but probably it does."

J., *c* II, 6 *Nud* (*Miz*). "The picture as seen in the experiment was visualized. The upper and lower parts resembled small cupolas on buildings."

J., *c* IV, 5 *Laj* (*Wej*). "It called to mind a picture with narrow green and brown stripes." After writing this, the subject complained that the picture was perfectly clear but that she found it difficult either to describe or to draw. In attempting both, she was forced to analyze the complex.

5. *Deliberate Analysis*.—This is not to be thought of as a sort of catch-all for protocols which cannot otherwise be classed. There are many, many such, but they have been quietly put aside; there is no pretence in this study of either finality or completeness, hence no need to force classification beyond the

point necessary to render the data intelligible. The protocols here dealt with form a very definite class; though, as with all the classes, the division lines are sharper than any individual case warrants. The analysis in all these cases is proposed by the subject to himself as a thing to be desired, and is deliberately and reflectively made. The questions arise: what suggests to the subject that he make an analysis, and how does he know where to put the lines of cleavage?

The nature of the perceived situation largely determines the latter, and our experiments do not offer a sufficient variety of situations to enable us to discover any uniformities. In some cases, of course, the lines of cleavage are already partly marked out by the selective processes of sensation, attention, or memory, as above outlined.

To the former question, the protocols return a better, though still incomplete answer. There seems to be an infinite variety of things which suggest an analysis. One is simply habit. Some people, especially those accustomed to scientific or philosophical thought, attempt an analysis of practically every novel situation in which they find themselves. This appears, however, less an independent suggestion than an aid to the others.

Another very important suggestion comes from memory habits. Quite independently of psychological proof of the fact, many persons have discovered that they can remember concepts better than images, the elements of a situation better than the total impression. Take, for example, the following general report by subject S. offered at sitting XIV (Group *c*):

"There came to me at about the second or third member of the second exposure the query: 'I wonder if there is not an easier method of impression—by grouping, say.' I then ran my attention back over the members of the series which had been exposed and noted color. This was rejected because no two forms seemed to be the same. Then syllables, some of which I remembered were repeated; but there seemed to be no clue by which to associate them, nothing the same except the syllables; so I gave it up for the time being and concluded without a group."

Another subject, Mn., seems to have given a quite individual twist to this. His procedure was to liken each syllable to some well-known word, then to find in the picture some feature corresponding to that word. Thus, syllable would call up word, word feature, feature entire picture. Needless to say, the task required considerable imagination for its fulfilment. For our present purpose, however, the point to notice is that, in the service of memory, the subject analyzes the presentation.

c III, 5 *Yol* (*Wej*). "Then I immediately remembered that when I saw the syllable a few minutes ago I connected it up with the word 'yawn'. Then I tried to associate the word 'yawn' with the picture which was given, and I remember now that it was the picture of a revolver. I tried to make that picture of a revolver look like 'yawn' during the presentation of the series, but I could not succeed; but I remember very distinctly the attempt. I do not recall the syllable which occurred with *Yol*."

c III. 3 *Wer. Fid.* "I tried to find something in the picture with which it occurred that resembled a wedge. I tried to find some visual association that would help me to remember the syllable when it should occur again in the part of the experiment that is now going on, but I can't recall the picture; I can only recall that it was very difficult for me to find anything about it that resembled a wedge. It had too many curves. The syllable is getting mixed up in my mind right now with that picture that resembled a revolver."

c IV. 4 *Kuc Gez*. "I immediately remembered this syllable as one that in the former experiment had occurred with the syllable *Bef*, and the picture somewhat resembled a piece of beef under a microscope. When the series was first begun, at the beginning of this hour, this syllable, Kuc, was presented to me in combination with another syllable, Gez. They both occurred with a picture which somewhat resembled a letter Y. The picture was built up in three sections, dark gray in color. I wondered, at the time, when the picture was first presented to me, why the syllable had been changed. That made me take particular notice of what syllable it had occurred with and the kind of picture it occurred with."

The last of these as a mixed case will serve to introduce us to the next form. The subject is conscious of changes: perhaps he notices the lack of a certain feature where his past experience led him to expect it, perhaps he simply realizes that there has been an alteration. The result is the same, an analytic examination to discover what is changing, what is constant.

P., c VI, general: "It has seemed to me that while the shapes of the pictures remain the same, the position of the different shapes changes somewhat and the colors are not always remaining the same for the same shape of pictures, though the same shades of colors seem to be present in the different experiments. The syllables, too, seem to be changing somewhat to make new associations."

A single protocol from subject J. also clearly belongs here.

c X. *Nud Miz*. "The figure within the square made me think of a butterfly, but its red color was inconsistent with that object." Here the subject is struck by a resemblance, but notices also the incongruities. This leads to an analysis to make clear the points of likeness and difference, in this case shape and color respectively. There is small doubt that there are many other suggestions which might lead to deliberate analysis, but these are all that gave evidence of themselves in the protocols.

Analysis would seem, therefore, to be furthered under our experimental conditions by cues depending upon the occurrence of such functions as attention, memory, association, linguistic or allied characterization, and deliberate intention. That the list is complete, even for our experimental conditions, is not contended; but a careful sifting of the protocols fails to bring others to light. Clearly the attention and memory cues belong together as aspects of the selective nature of consciousness. The different features of the presentation have, so to say, different values for consciousness. The position is not clear of the third mentioned, where that element of a presentation which acts as the associative link is subjected to analysis; but on the whole it, too, would seem to be a case of "apperceptive" effectiveness of such a presented feature. Language and allied symbols

are so essentially social that the cues to analysis through "characterization" might even be termed the social cues, even in those cases where verbally-minded subjects characterized—and thus analyzed—without reference to the necessity of making a report. The apparent self-determination of consciousness (which must be accepted as a phenomenological datum) sharply distinguishes the last cues from all the others.

It may not be from the point again to call attention to the somewhat artificial nature of this classification. The necessity to characterize and the determination to analyze can never of themselves lead to analysis since they do not indicate lines of cleavage. On the other hand, the cues of the "selective" group do indeed mark out division lines, but do not suggest that these be followed. In short, analysis is essentially a single, unified process. Yet one must not neglect the fact that in the one case the emphasis in the subjects' protocols was upon the lines of cleavage; in the other, upon the need to follow them. Nothing that has been said above is intended to convey the idea that any of the cues is exclusive of the others. So little is this the case that the chief difficulty has been to disentangle them sufficiently to render classification, and hence intelligibility, possible.

The Cues to Conception

The inveterate tendency to apprehend facts in various relations (which seems to be the fundamental impulse towards analysis) may be satisfied not only by analysis, but also by conception. It will come as a shock to some to find conception put at the beginning instead of the end of a process of abstraction, and the question will at once be raised as to what we mean by the term. We mean that the presented object is perceived *as* something, is thought of *as a case or an example* of something, is assimilated to certain mental categories. By whatever name it be called, this is often a first step to abstraction.

If we bear in mind, as we must always do, that the subjects of this experiment were adults, the statement loses its mystery, even for those who are committed to the view that our concepts are reached by a process of abstraction. Our subjects were able to (and did) use constantly the concepts attained, if you will, by previous abstractions. The object is conceived as a member of a certain class. Now in strict logic, it belongs to a class only by virtue of its possession of certain abstract qualities, and it is very easy for an arm-chair psychologist to imagine that some sort of analysis is presupposed whereby these qualities are separated from the others. Such analysis, however, gives no evidence of its presence in the subject's consciousness. If anyone thinks that he makes the facts clearer by saying that the analysis takes place subconsciously, we shall not object. We merely

insist that this analysis is not, in these cases, part of the abstraction process as conscious fact. Our problem at this point, then, is to discover the cues or motives which lead the subject to assimilate the object perceived to some concept.

This assimilation is rather "rough and ready", apparently "intuitive." (We shall see that the intuition is prepared for.) In defining more carefully the exact conceptual meaning of the object, an explicit analysis often becomes necessary. But the analysis in this case follows the conception, whereas, in the cases just considered above, it precedes. In what follows, it has been assumed that essentially the same sort of process is involved when a certain feature is treated as a representative of the whole situation, and when the whole situation is taken as a particular instance of a more general category. This has been the traditional view and Avey (4) has recently found justification for it.

1. *Assimilation by Translation into other Sensory Modalities.*—It is, perhaps, only in those cases where the two senses must present the situation in quite widely different terms that translation from one to the other leads to assimilation. The whole process can be made clear by a concrete example. The common element which stood in one-to-one relation with the term *Bekis* in Group *a* was "circularity" or, more accurately, "curved geometrical figure-ness." Upon being shown *Bekis* for reaction in the second reaction period, subject W. wrote: "I had a motor image, which was that of moving my hand in a circular direction." She had seen with *Bekis* a circle and an ellipse. Her motor imagery was not fine enough to represent the differences between these two figures. Instead it could only represent the grosser aspect common to both presentations. Through the translation to motor imagery there has occurred precisely that emphasis upon the common element of two objects which leads to their assimilation to a common class.

The same subject began a very interesting development when she first saw one of the flower pictures called *Fefiv*. The particular flowers were hyacinths. She writes:

*W., *a* I, 2. "I first had an auditory image of the word fragrant, as I had said this to myself in connection with the learning series. I think I had a slight olfactory image."

A little later this passes over into a distinct olfactory image.

W, *a* III, 3. "This first gave me an olfactory image of the smell of flowers (I can't tell what kind)."

*W., *a* VIII, 7. "I had an olfactory image of smell of some kind of flower. Then I had an idea of flowers in general which seemed to get me all confused until I finally got the visual image of the apple-blossoms."

This general meaning of *Fefiv* as fragrant develops to the point where a presentation is rejected as a proper associate because not fragrant.

*W., *a* VII, 2. "I immediately got a visual image of golden rod. I had a feeling of dissatisfaction because I do not think golden rod goes with *Fefiv*; for the flowers that I connect with it are all particularly fragrant ones."

The process may seem to be analytic and to belong to the cues given above. As was pointed out there, however, whether a given case is analysis or assimilation depends upon the subject's attitude. If the subject regards "fragrant" as one of the many qualities of the object and seeks the others, we have analysis. In the cases here cited, however, this is not the case. Fragrant is looked upon as expressing the essential quality of all *Fefiv* situations, so that, as in the last protocol quoted, anything not fragrant is rejected.

Nearly all of the examples of this cue appear in the protocols of this same subject (W), who is the only one with a rich outfit of imagery in practically all the sense modalities. One may note also a protocol from An., *a* XVIII (*Cipuj*), which seems to belong here: "The filling pattern seems 'full', has tactual value." The complexity of this pattern was, as a matter of fact, its chief distinguishing mark.

The following two quotations from J. show incipient assimilation which presumably met with negative instances, since it does not develop.

c VII, *Wej* (*Dut*, *Yol*). "I remember that during the presentation period, the picture (perhaps its color) gave me the feeling of something cool."

c X, *Bef* (*Laj*). "During the reaction period (not upon first presentation) I followed the outline and thought of a dog at home."

2. *Assimilation by Translation into Feeling Terms.*—The essential principle is almost exactly the same as in the section just preceding. Not only this, but it is doubtful if some of the protocols here quoted do not belong there. The subjects are not unlikely to use the term 'feeling' rather broadly. We have accordingly brought together here not only those cases where the general meaning consists of feeling in its strict sense, but also those cases where the general meaning is so vague as to be described quite untechnically as a "feeling." This feeling stands as the general meaning of the exposed word, just as the olfactory imagery of fragrance or the motor imagery of a circle stood for the general meaning of their words. In short, we are dealing here again with the representative function.

An., *a* III, *Fefiv*. "First came a feeling-tone of lightness and delicacy attached to the term; then the verbal image 'flower'; then the visual-verbal image of the lilies."

It is uncertain whether the next two protocols belong here or not.

*S., *b* III, *Tugic*. "In kinaesthetic terms, 'that meaningless, unmusical piece', so designated since I was unable to catch a single word, and the roughness of the auditory impressions was decidedly unpleasant."

J., *c* V, *Yol* (*Miz*). "The sight of *Yol* made me associate it with a large, massive, strong object, which brought to mind the elephant." Here the association is apparently due to an organic kinaesthesis-complex, only with difficulty distinguishable from a feeling. A hint of one way of forming analogies is given us.

3. *Assimilation by Apperception of Use.*—The functional psychologists have done good service in calling attention to this process, though their insistence upon it as the essence of conception and abstraction is certainly one-sided. Use is a more general category than the individual percept. We may use quite

[illegible] is involved. The [illegible] certain [illegible]

[illegible] of this sort of [illegible] instruments [illegible] There were no [illegible] examination [illegible] to the [illegible] it seems [illegible]

[illegible] and the [illegible] section.—The [illegible]

[illegible] reverberations [illegible]

[illegible] immediately [illegible] should [illegible]

[illegible] at the top [illegible] should [illegible]

[illegible]

[illegible] but when [illegible] the [illegible] is altered [illegible] the following protocol. [illegible]

[illegible] feeling [illegible]

[illegible] order to render [illegible] more [illegible] as we saw above, [illegible] method. In the [illegible] the subject selects some [illegible] the most essential [illegible] were the [illegible] interpretation, but not [illegible] the attitude of [illegible] two cases. In true [illegible] a small [illegible] feature. In the cases [illegible] avowed purpose [illegible] the [illegible] feature is [illegible] make the [illegible]

W., *a* XIV, *Dojaf.* "I had a visual image of a little white circle, and immediately I knew it was a graphophone, for this little white circle was on the horn of the machine."

W., *a* XVI, *Wocag.* "Then I tried to get a visual image and all I could get was something like this: two lines forming a right angle, although it meant rectangle to me."

Av., *b* VI, *Livab.* "Piece which starts with a whistle; I don't know any name. I find myself looking for something to hold to for identification."

S., *b* III, *Xugic.* "A more definite kinaesthesis appeared, to indicate a particular piece whose name I do not know but which was, I think, a quartet."

S., *b* VII, *Livab.* "The reference was to one of the phrases in the song which I could make nothing of except 'Wiggle de cag'."

Subject J. writes, in *c* VI, *Kuc,* of "the rectangular one with cut-out sides;" and later, in *c* XVI, *Kuc,* "the cuts in sides of picture in the image as also in presentation period seemed to me to belong to *Kuc,* and *Kuc* to them."

Mn., *c* X, *Bef* (*Laj*). "I know that it was a picture that was a brownish color and had a little figure of a rose-bud in the center of it. I am quite sure of that, for I have been in the habit of associating the syllable before with that little red figure in the center of the picture. I remember having recognized that this morning."

A definite sub-class is constituted by those cases where the blurring of the image leads to conceptual assimilation. A vague, confused, or incomplete image can be identified only as belonging to a certain class or group. Its individuality is lost. Although "logic-mongering" psychologists have often talked as if this were the only type, comparatively few cases occurred. The subject cannot at first identify the "fragrance" as the odor of any particular flower. The motor image is too crude to represent a circle or an ellipse; it represents curved lines in general. Many of the "feelings" are almost certainly such vague images in complex settings. We have brought together here, however, chiefly those protocols in which the assimilation is not helped, as in these cases, by other cues.

*An., *a* X, *Dojaf.* "Prompt meaning of musical instrument, not in visual-motor verbal terms, but a faint kinaesthetic attitude towards a semi-circle of schematic instruments surrounding me, *very* schematic and faint. The image of the piano came, rather non-significant, just as I reacted."

*An., *a* X, *Xugic.* "Blank with mere recognition of the term as such. Then the visual motor verbal image (fruit). I then reacted; but as I was reacting the real meaning 'butted in' carried by a visual image of some sort of receptacle and thus of the bucket. So far as these new meanings come, they seem to come by means of a fading away of the outline and colors into the fringe, leaving the pattern in bold relief."

C., *a* III, *Dojaf.* "Reacted to verbal imagery 'musical instrument' and vaguest sort of visual image of the instrument (I don't know what)."

W., *a* IV, *Hexur.* "My idea of soldier seemed to consist in a visual image of a soldier's arm supporting a gun; the rest of the figure is gone."

W., *a* VII, *Vupaw.* "It consisted in a decidedly mixed-up mass of visual images of different fruits and some gustatory images of the same."

5. *Assimilation to a more General Class through Failure of the Particular.*—Only three good instances of this class were found, but all are quite unequivocal.

[illegible] not a visual image of the geometric figure; I [illegible] an appreciable time before I could [illegible] visual verbal terms.)"

[illegible] The image was a [illegible] difficult to name, [illegible] Before she could [illegible] and its con[illegible] simultaneously.

[illegible] geometrical figure [illegible]

[illegible] The Seitz-figure [illegible] any name. [illegible] chiefly by [illegible] concrete visual [illegible] Hence a failure [illegible] meaning of the general [illegible] the cues [illegible]

[illegible] geometry [illegible]

[illegible] more fre[illegible] experimental [illegible]

[illegible] and [illegible]

[illegible]

An interesting sub-class, if not a special group, is constituted by the following protocols. In all of these, the syllable recalls two images, both of which can be designated only by their class name. No doubt the presented object, the picture, had been assimilated before the reaction, though we cannot be sure. It does not follow that there is no gain. The simultaneous presence of the two images, and their designation by a common name and by a single nonsense-word, prepare the way for the nonsense-word to become the name of an abstract quality.

*W., *a* IV, *Dojaf*. "A visual image of a grand piano, followed by a visual image of a victrola (with a horn). Reaction given with image. The idea of musical instruments seemed to follow the two previous images."

*An., *a* IX, *Vupaw*. "Fruit. Schematic visual image of a pile of mixed fruits apparently brought meaning. Later came particular image of bananas. The term 'fruit' came, I think, in visual and motor verbal imagery terms, but very fleeting and faint."

Still less than in the case of the cues to analysis is it pretended that the above cues to conceptual assimilation are exhaustive or adequate. The greater number of the experiments were designed to bring out not the conceptual but the analytic approach to abstraction. Fortunately, since there are thus fewer examples, the types are more sharply defined, there is less overlapping. Yet that the above classes represent more than divisions of convenience can scarcely be maintained. The fact that these divisions *are* convenient, however, shows that the classification is not entirely arbitrary.

Nor is overlapping entirely absent. The first two sections clearly belong together. The nonsense-word is associated with something other than the primary data, with other kinds of sensory images or "feelings." This leads to conceptualization by emphasizing the only quality which unites the two forms of presentation, which is common to both because they are both instances of it.

The cues of the next section (through apperception of use) have this in common with the first two, that they too deal with transformations of the primary data, and that the transformation again brings out that which makes it possible—a more general category.

Blurring of the image and representation belong, similarly, to another group. In these two some one aspect of the primary data is emphasized. In the one case, it is the only clear or "visible" aspect, in the other it may be one deliberately chosen as the representative. In both cases, the emphasized quality is one which unites the presented object to some relatively stable mental category. The two forms often cooperate. Standing between these two classes and the three previous are the cases where a verbal term acts as representative of the primary data. For the name is distinctly "felt" to be a representative rather

than a transformation of the presentation, so closely is it connected with the objects it symbolises.

All of these classes are of such a nature that, although they involve a developed consciousness with pre-formed mental categories, yet they can assist in developing new categories or in modifying old ones. In the cases of Assimilation through Failure of the Particular and by Immediate Association to a Class Name, this is not true. The cues lead the particular over into the general, but do not take any part in modifying either. They presuppose very definite classes, and classes which moreover have previously included just such objects as the present ones. These two classes, therefore, are clearly much less fundamental than the other five.

A noteworthy point is the relative lack of importance of language in this conceptualization as compared with analysis. A casual reading of the logical treatment of conception would lead to the belief that concepts and terms are identical. And while this extreme position is probably held by few, language is considered a fundamental part of the process by practically all logicians. It can be seen from the above that such is not the case. In most cases, its functions are the strictly secondary, although still very important ones, of clarification, definition, and fixation of concepts. Only seldom did it play a great part in the modification. The point illustrates not so much the divorce between psychology and logic as the necessity for psychology to make an independent study of processes also dealt with by logic.

The Further Development of the Process

The development of the process of abstraction is necessarily somewhat different, according to whether the first phase ended in conceptualization or in analysis. Conception is obviously very much nearer to a complete abstraction, so much so that at first thought they seem to be identical. But they are sharply divided in fact by a difference in the subject's attitude.

In the one case, the subject views a presentation more or less in its entirety as a member of a certain class, which class is named by the nonsense-word accompanying it. The concrete details of the situation are far from uninteresting; it is rather the concept which is dull. One subject (W.) complained of the *Hezur* (soldier) group that diversity was almost lacking, while in the *Fefir* (flowers) group there was an interesting variety. The same subject explains that she is not satisfied with knowing the general meaning of the term, but wants always to know what particular *Bekis*, *Dojaf*, or *Hezur* was shown in the current sitting. It can hardly be said, therefore, that there is an abstraction of all save the common concomitants of the given word, although the word is clearly the name of a class-concept.

(Compare the remark of W. in *a* XVI: "I believe if I were to meet a soldier on the street, I'd call him a *Hexur*; *Hexur* and *Fefiv* are now regular words for me.")

Yet we have said that conceptual assimilation may serve as a first step towards abstraction. In this way: the "nonsense-word" becomes firmly associated through such conceptualization with precisely the only relevant quality. The abstraction does not take place, however, until the irrelevant is recognized as such, and this is a step in advance of assimilation to some mental category. How that step is taken, what are its cues, to extend the term used above in a slightly narrower sense, the data of these experiments do not enable us to determine completely. Of one thing we may be sure, that relative familiarity of the concept is unfavorable to this change of attitude. Where the concept is easy, familiar, simple, the attention of the subject is turned towards differentiation; where, on the contrary, the concept is relatively new or recently modified, where its boundf aries are as yet but ill-defined, the subject will be impatient o- all save the essential qualities of the concept, for it is these which will serve to differentiate it from others. Hence we find, with subject W., that the very much more difficult new material of the second half of Group *a* leads to an abstraction from the non-essential details in half the time taken for the easy, familiar objects of the first half.

Another factor which helps to change the attitude is what we shall have occasion later to consider under the name of "associative abstraction." The subject finds himself unable to recall aught save the essential quality. This is not yet a full-fledged abstraction, but if he recognizes this *as the essential quality*, he is led to think: "After all, what does the rest matter?" These are the only hints of the cues which cause the change in attitude from conception to abstraction. It will be remembered that conception as the first phase of abstraction was predominant only in the protocols of Group *a* and was not there exclusive.

We can trace the change from analysis to abstraction better in the protocols of Group *c*. In the case of only two subjects did this change spontaneously occur. We must take up their protocols in some detail; but before we do so, a general statement will help to orient us in that study. The change from analysis to abstraction is sudden, apparently intuitive. Now to call a change 'intuitive' is to challenge the psychologist to trace its genesis. We must seek, then, for changes in the protocols which belong to the level of analysis, but which tend to lead the subject away from that level.

We can trace the process most easily in the protocols of subject W. This subject did not approach the experiments of

Group 3 quite unsophisticatedly. She had been subject in Group 2. More important than this, she knew that the "pictures" were to be made out of wall paper. The latter fact provided her with a ready-made, though not quite complete analysis. The former fact influenced her reactions in several ways. First it undoubtedly caused her immediately to look upon two syllables as the name of the picture with which they were given. That at first the two syllables were considered to be a unit, one name just like John Brown, is obvious from her remark at the end of the first reaction period: "It is very clever of you to make us remember twice on each picture." That is, each syllable was felt to be the equivalent of one of the nonsense-words in Group 2. This substantiates the subject's subsequent report that her first effort was not to keep the two syllables together.

Her familiarity with Group 2, particularly with the last three sets, also undoubtedly helped her in her analysis of the figure. It helped her primarily, it seems to us, by providing her with the appropriate categories and thus suggesting the lines along which her analysis should take. True, all of the subjects have such categories as pattern, color, form, but they do not bring them into relation with the experiment so immediately. In almost every one of 6's protocols one or the other of these categories is used. In the main, this seems to have been relatively determined, but it was certainly aided by the memory [illegible] analysis and to no small degree by the [illegible]. Two things were particularly striking: the little figure in the [illegible]-pattern, and the red of the [illegible]-pattern. The [illegible]-pattern was also fairly well noted, but the other pictures were merely "those brown ones."

The process as it took place in the first sitting seems to have been as follows: first, a [illegible] analysis as attention passes from aspect to aspect according to the regular laws governing it; secondly, the assimilation of the aspects thus separated to various mental categories (largely provided by the experience of Group 2) and the adoption of the analytic attitude, the "set" to apprehend the presentation in more or less elementary pieces. Of course these two processes are not temporally separated: [illegible] the "fixing" of the analysis by differential memory for various elements and by the necessity of making a report.

During this first phase, however, the subject has no idea that the pictures are artificial synthetic products of these elements. They are thought of as unities which decompose into, rather than are composed of, the elements. And it is essential to notice that at this stage the two syllables are regarded as really one, a name applying to the pictures as unities.

The next step is a change in the attitude towards the syllables. The effort to keep them together breaks down. The subject is forced to recognize their mutual independence. This process begins in the second sitting. Certain of the combinations are remembered from the previous sitting, especially those making rhymed or otherwise euphonic combinations, such as *Wej, Laj*. This combination is not met in the presentations. Instead another context is found for each syllable. For instance in sitting II, *Wej* appeared in two combinations as *Wej Kuc*, and *Wej Dut*. The subject writes on being shown *Wej*:

"First an auditory verbal image of *Wej Laj*. I then said to myself that those went together in sitting I; but I was very doubtful about their being together this time. I felt that there was another word in place of *Laj*, but when I tried to think of it I did not succeed. *Wej Laj* just rang in my ears, and it didn't seem as if anything else could go with it."

Or again, when shown *Laj* a little later in the same sitting:

" 'That goes with *Wej*.' An auditory verbal image of the two. Also a visual verbal image. I said to myself, 'they go together anyway, even if I didn't have them today.' "

Simultaneously with this there has started another process relating to the syllables which is of great importance. Merely as an aid to the recall of the other syllable, the subject asks herself what was the position of the given syllable in its pair. Even in the first sitting this device was three times resorted to, and in the second it becomes a part of the regular procedure.

This gradually alters its nature. Instead of merely "first in its pair," the syllable becomes "one of those syllables which come first in the pair." The step, which is a most important one, seems to have been taken towards the end of the second sitting and during the presentation period of the third. With this subject, we have no evidence in the protocol how the transition takes place; the classification appears suddenly as a fairly-well established hypothesis. This hypothesis outruns any substantial inductive basis for it, but was explained by the subject at a later period as due to a desire, which could not be repressed, "to beat the experimenter's game." If the syllables could not be kept together, evidently the experimenter meant to keep them strictly apart. Then on what basis? For the first two sittings certain ones were first, others second. Very well, this would serve provisionally for classification. In line with the general plan of the whole research, to give the subject few instructions and to approach as much as possible the conditions of real life, no negative instructions were directed against this attitude.

Having now two classes of syllables independent of each other, what more natural for one who takes this attitude than to seek the things to which the syllables refer? For we have seen that they were from the start regarded as names. Since they cannot

be jointly one name, they must be severally two names. At this point again we must see the very great influence of the subject's previous knowledge. She knew, as has been said, that the pictures were cut out of wall paper, a fact which suggests that the shape and the appearance of the paper are important items *as elements either to be associated or to be neglected*. For, in Group *a*, certain patterns whose color and outline varied irregularly had been given a nonsense-word name, while other nonsense-words were applied as names to outlines (not generally the same as used with the patterns). The subject thus has the categories to which nonsense-word names have been applied by the same experimenter under similar circumstances, and her analysis of the presentations has provided instances of these categories. Pattern and color are not always sharply distinguished from each other but are collectively called "wall paper." In the first protocol, however, where the hypothesis appears, the contrasted elements are pattern and outline.

III *Wej* (*Yol*). "First I remembered that this syllable came first. Then I tried to think of the other one, too. I had a visual image of *Yol*. Then after that I had an auditory image of the two syllables. Then I tried to think of the figure that went with it. I said to myself 'That first word has something to do with the pattern of the paper, not the shape of the figure', so I tried to think which wall paper was used. I got a visual image of wall paper made of fine lines going horizontally in gray and yellow. Then the image seemed to take on an outline rather distinct, which makes me think somewhat of an elephant. Then I said to myself 'No, that isn't right, for if it is one of those papers it was covered with either fine pink or fine blue lines', but when I thought of those it did not seem as if they had gone with the word *Yol*, so I thought again of the first image I had had. This seemed right for *Yol*, but I was not sure of it for the other word. However I reacted."

Questioned as to why she thought that this syllable was connected with the pattern rather than with the figure, "the subject could not give a satisfactory reply but added, 'I was beginning to form some associations now between the last syllable of the two [in each case] and the shape of the figure'."

It is impossible from the protocols to trace the exact genesis of this hypothesis. Kant seems to have meant this sort of thing when he spoke of the representation of a method, Husserl calls it categorical intuition (kategoriale Anschauung), while Bühler (5) has subjected it to some degree of experimental analysis under the name of "Regelbewusstsein." Yet no one has, so far as the writer is aware, directly attacked the crucial problem of what it is that leads us to propose certain hypotheses to ourselves for experiential testing. It is precisely because our own experiments enable us to show only *that* and not *how* analysis and conception pass over into abstraction that this study has been limited to the initial phases of the last-named process.

We may be permitted, however, to watch the development a little further. One interesting point is this subject's procedure from class to member of a class.

W., III, *Jid* (*Laj*, *Nud*). "First I said to myself, 'It was probably one of those brown ones?' Then I thought, 'No, today the papers were mostly colored.' Then I had a visual image of the pink paper."

W., *Gej* (*Yol*). "First I said to myself, 'Now that was the last syllable.' Then I decided that it wasn't the last syllable, but was one of the first, and so referred to the paper rather than the figure. Then I had a visual image of the red wall paper. Here I reacted. After reacting I got a visual image of the wall paper in this form [drawn]."

W., VII, *Gej* (*Nud*). "First I said to myself, 'Now that syllable goes with the wall paper'. Then I asked myself what wall paper it was. First I thought of the wall paper that is made of rather broad stripes, some of it is red and some of it is brown. Then I said to myself, 'No, that isn't right.' Then I thought of the wall paper in which the figure is made up of fine gray and pink or gray and blue stripes. But I said again, 'No, *Wej* goes with that wall paper.' Suddenly I had this visual image. It was a blue wall paper made in this form: [drawn]. Then the paper seemed to be in this shape: [drawn]. Here I reacted. Just as I reacted I got a visual verbal and also auditory image of the word *Laj*."

Another point of great interest is the fact that although from the third sitting on the subject is always aware that a certain syllable refers to or names only a certain element, negative abstraction takes place to only a small degree. It will be remembered that Külpe (10), Grünbaum (8) and Moore (12) all found that the greater the positive abstraction, the greater the negative. This was due, however, to the common experimental procedure of all three. The result of their instructions was in each case the more or less conscious "Aufgabe" at work upon material possessing little or no organic unity, and could not fail to result in a positive repression of the "unessentials". Our subjects, being left to themselves, followed their own bent. Subject W., while recognizing perfectly the one-to-one relation of the syllable and the presented element, does not consider the other elements as uninteresting and constantly endeavors to recall them.

The case must not be confused with the same subject's tendency in Group *a* to recall all of the various individual "pictures" with which a syllable had been presented. This we denied to be abstraction on the ground that the attention was on the denotation rather than on the connotation of the syllable, as was there required for abstraction. The subject's attitude was perceptual rather than abstractive (see above, p. 38). In the cases now under consideration from Group *c*, the subject is very much interested in knowing what other syllable and pattern had been present with a given shape, say *Kuc*, but these accompaniments are clearly recognized as having no influence on the nature of the *Kuc*-outline or on the meaning of the syllable.

The other subject whose insight came very quickly was El. He started with no such foreknowledge of the problem as W., but he seems from the first to have been unable to repress a tendency to speculate. In the fifth sitting, it became very obvious that his reactions were no longer unsophisticated. Accordingly, after the ninth protocol, he was questioned as to the reaction for the association he had reported.

Question: "What makes you think that *Kuc* is to be associated with that figure?"

Answer: "Because during the initial exposure today a hypothesis, which I have been suppressing, became very dominant, namely: that the second of the two syllables is the one which means the figure associated with it, while the first of the two syllables means the color of the same. It is evident that in all the introspections given today this 'Aufgabe' is controlling me. The hypothesis appeared today very suddenly, as if the

thing had been forming in my mind unconsciously since the previous experiment day, and not at all as a result of this morning's exposures. These exposures, however, have not served to disprove the hypothesis, even if there is considerable doubt of its proof in my mind. This came so suddenly and with such a feeling of success that I am reminded to add that it was present on the first exposure of the series, *i. e.*, of today, and was not at all the result of a deduction formed during the exposures this morning." Later he spontaneously adds: "As near as I can get the first flash of insight, it consisted in remembering that the *Yol*, particularly on the very first exposure of the first series, had always been associated with the 'polar bear'; secondarily, but far less near, the association between *Miz* and the brown with the faint green stripes running through it seems also to have played a part in forming this definite conviction that this is the 'Aufgabe' controlling the experiment."

This reply serves to orient us as we take up the detailed study of his progress from day to day. Especially it behooves us to take particular notice of the protocols concerned with *Yol* and the *Yol*-'polar bear' figure. Of the presentations in the first sitting, little was correctly recalled in the reaction period except *Yol*. The *Yol* outline had been seen twice with *Bef* and with *Miz* patterns, the former being the first presentation of the whole group. When *Yol* was shown for reaction, a fairly complete report was given; *Bef* is recalled to the left of *Yol*, the figure is likened to a polar bear, the brown color is prominent. *Miz* recalls *Yol* to the right with the brown polar bear in the background. *Bef* recalled *Yol* (no order is mentioned) and a visual image of the brown polar bear of which the "head" is clearest.

In the second sitting, *Yol* was seen in a *Gej* pattern of a striking red color. *Gej*, however, fails to oust *Bef* as the associate of *Yol*, but the color of the "polar bear" fluctuates between brown and red, finally settling on red. This sure association of *Yol* with the "polar bear" figure together with uncertainty as to color is very significant. The dissociation of color as being referred to by *Yol* distinctly begins. To some extent in sitting I, and very noticeably in II, the analysis of the pictures is proceeding by the cues of Differential Memory amd of Characterization. *Miz* is incorrectly associated with a *Wej*-pattern.

In the third sitting little that is new develops. Few of the syllables recall correct or at all accurate associates. *Bef* continues to be recalled as the syllable associate of *Yol*. The combination of *Bef Yol* is firmly fixed as the associate of the polar-bear figure which appears with "various colors competing." It is strange that with *Bef Yol* so firmly established, no shock was felt when *Wej Yol* was seen. The analysis by the various cues, chiefly Memory and Characterization, has proceeded so far that "form" is distinguished from "color," but "color" is used for the colored pattern.

Sitting IV shows notable advances. At first *Bef* is recalled by *Yol*, which also recalls the polar-bear figure with alternative

colors. When *Bef* is shown, however, the subject recollects that in spite of his immediate recall of *Yol* and the polar-bear figure, *Bef* has been "in an association with other syllables with other figures, but am unable to say which." This is a point of very great importance. The *Yol*-figure is already clearly separated from its accidental concomitants; but, so long as *Bef* and *Yol* are kept together, the abstraction of all other qualities except a certain outline as associates of *Yol* could not take place. Meanwhile, as with subject W., a division of syllables into left-hand and right-hand syllables has been effected. The subject is getting more familiar with the pictures and recalls them more accurately.

Sitting V was the critical one, as the subject's general report cited above indicated. The *Yol*-outline was again the first one shown. *Yol* had in the four preceding sittings become firmly associated with this figure and in the last had thrown off *Bef* as a constant associate. When the figure reappeared in a "new" color and with a "new" syllable, the supposition that the last syllable corresponded just to this outline became unavoidable. We may discount the subject's insistence that it was present at the beginning of the sitting, though some vague inkling may have indeed been present. From *Yol* as referring to a single outline is but a short step to a complete schematization, since as Bühler (5, 338) has shown, we form "Regelbewusstsein" with extraordinary facility. All the necessary facts are in the subject's possession: two classes of object-elements, two classes of syllables, and in one case at least a well-established one-to-one relation between a member of one class and a member of the other. The subject somewhat hesitatingly ascribes to a fairly constant association in his mind between *Miz* and its pattern some part in building up the hypothesis. This is so far from being true that *Miz* had previously never recalled its proper pattern. The hypothesis is thus based largely on a single instance *plus* a vague consciousness of the other instances. The subject at once sets to work to test this assumption by the method of agreement and difference, and finds, on his own showing, at least no disproof. In the fourth and fifth protocols of sitting V he announces his conviction that in each case only a color or only an outline is relevant. At the next sitting, two examples of one pattern in different colors bring home to the subject the difference, of which he was already half conscious, between color and pattern. With the formation of this hypothesis, abstraction has clearly taken place.

As before, however, it is worth while to notice a few further points. With this subject, the procedure from class to member of class seems not to be so common as with W., and one may add as with An. Rather, as he says later:

"I appeared spontaneously to wait for the appearance of visual images. When the reference of the syllable is to the left, the first sort of image to appear is usually that of the pattern. Sometimes it inhibits the appearance of any form image and sometimes it doesn't. Usually the pattern image appears before I get the realization that the syllable refers to it, and not *vice versa*."

To the extent that this is the case, it is probably due to the fact that the syllables were less clearly or rather less emphatically divided into two classes. Before he realized to what they referred, Subject An. greeted each syllable with a kinaesthetic jerk, placing it to the right or left, while W. reacts with the verbal kinaesthesis "first" or "second". Neither of these tendencies is found in so pronounced a form with El.; the position of the syllable until after the hypothesis was formed was rather an indifferent detail. Even so, the procedure from the consciousness of the class to the consciousness of the member of the class is by no means uncommon.

E. V. *Gej Lef.* "Introspection shows this 'Aufgabe' governing my search for a color to be associated with *Gej*. This search was unsuccessful unless it be the case that the finally found color, red, is the one which seems to be most probably the correct one. The process of search consisted of running over in my mind the various colors with rejection. Such colors seen were the blue and silver stripes, the brown with the squares of black lines, the brown with the light green, irregular markings and, finally, the bright red. In the background of my mind was the phrase, 'episodic connections' (between these various colors and the figures involved). In particular the blue with the silver stripes was associated with the vest-shaped figure as I think they were given in the exposure this morning."

[illegible] "The first datum was the knowledge that this syllable is the second of a pair. In recollection of the theory formed last time this immediately led me to search for the shape of the figure, which appeared very distinctly as the figure pointing toward the upper left-hand corner. The consciousness of a peculiar color in which this figure appeared for the first time this morning. The backward association to various syllables was rather prominent, &c., occurring repeatedly."

That the hypothesis or "Aufgabe" controls the subject's reactions in ways of which he is not himself conscious is seen from the following fact. Although the subject often made errors after this he never assigned a pattern word to an outline or an outline word to a pattern.

Negative abstraction becomes increasingly greater with this subject. Images of form are inhibited when the stimulus word is a pattern syllable. In part, at least, this is intentional, in part it seems to be automatic.

We may introduce here points gleaned from the protocols of subject An., who alone of the other subjects attained a fairly complete abstraction. Indeed, having advised the experimenter upon points of procedure, he knew the problem too well to adopt anything but the abstractive attitude, and all his protocols therefore were taken after the point chosen as the limit of our study proper had been attained. We have, it is

true, cited many instances of the cues to analysis or conception from his protocols, but these cues occur as parts of a finished product and could not be given their proper setting in the process if all the subjects had attacked the problem with the same degree of sophistication as to its nature.

It is interesting to notice, however, certain further developments. This subject practically always first assigned the stimulus syllable to one or the other class before associating it to a particular member of the class. In most cases, unless the class could first be determined, the individual associate was entirely inhibited. Negative abstraction was very marked. The subject seemed almost annoyed if he got a visual image of anything "irrelevant," an attitude in striking contrast to that of subject W. On the other hand, in those instances where a pattern-word recalls an outline-word, the latter controls the immediately subsequent reaction and the subject gets an image of a form, *then* of the pattern on the form. The contrary process is not found at all often. In other words, the subject can more easily abstract from the outline. As between color and pattern, decision is not so easy; but it seems likely that patterns, at least the sort of colored patterns used in this experiment, are more easily abstracted from than color.

The progress of the remaining subjects in Group *c* towards complete abstraction was much slower. This has, for our purpose, certain advantages which serve to offset the disadvantage that none of them finished in the number of sittings which could be given to this experiment.

The first is the bringing into clear relief of a process which would otherwise have remained obscure. It depends for its efficiency upon the law of frequency of association. Certain features have been seen with every exposure of a given syllable while other features varied. Thus is established between that syllable and that feature a stronger association, and though the others are forgotten, this one feature is remembered. We shall return to this process later.

The second advantage of the slower progress is that we may by comparing the two groups of subjects throw additional light on the mechanism by which one advances from analysis to abstraction. Differences in detail will be noticed when we take up the different subjects singly, but one great difference may be considered here. It is difficult to characterize it in a word, but one may say that the subjects who did not attain to abstraction attacked the problem *unspeculatively*. They were not trying to "beat the game," but merely endeavoring to make a report in accordance with instructions.

With subject P., the difficulty of making a report was sufficient to check any speculative tendencies for some time. His

whole effort was directed primarily to "finding" the companion syllable of the reaction word; next and less important, its companion picture. In general, his imagery reproduces the presentations rather poorly, even the verbal associations being more often wrong than right. The analysis by differential memory makes only comparatively slow progress since the subject strives constantly for total recall. By the end of the sixth sitting he has advanced only so far as to realize that the various shapes have different "colors" at different sittings, and that the syllable pairs are not constant. This last fact becomes of engrossing interest to him; his whole effort is devoted to recalling, and if possible dating, all the syllables which had ever been presented with the given reaction syllable. The subject explicitly states this as his "Aufgabe" in this sixth and the seventh sittings. Visual images are present only incidentally. It seemed that such exclusive preoccupation with the merely verbal must be broken up. Accordingly preceding his ninth sitting, the following comment was given him to read: "You are away off. You have been verbally associating one syllable with another, making a pair unit. Just remember that each syllable alone means something specific—which is not another syllable with which it may have appeared." The result was immediate. The subject reports:

"There seemed more of simultaneity in the coming of the syllable and of the visual image than ever before. The syllable and picture seemed to be there at once" (IX, first report). "In this instance there seems a strife between a verbal association with *Jid* as *Jid Dut* and a tendency to form the visual image with the single syllable *Dut*. There seems for the first time a tendency to run from the first syllable to the visual imagery" *Dut* (*Gej*). A marked improvement in accuracy of recall follows, in spite of rather poor physical conditions during this sitting.

Nothing in the way of systematization takes place, however, in the next three sittings; but two facts are laying the foundations for it. The reaction syllables in increasing measure call up the "picture" that day presented with them. This is due to several factors. The newness of the laboratory experience has worn off, all the presentations are familiar and, the excessive preoccupation with the syllables having been done away with, the subject is able to turn his attention to the figures. In some cases, although the picture as a whole is incorrectly recalled, the one particular feature referred to by the reaction syllable forms part of the visual image. Since the subject is as yet clearly unaware of any such relation, we must ascribe this to the associative abstraction of which we spoke above.

A second influence leading toward systematization is the recognition that a syllable has "gone with" two figures of different shapes but the same "color."

X, *Nud* (*Miz*). "There was present with it also the association of *Miz* with another syllable and with the same colored picture but a different

shape. The other syllable did not appear, but *Miz* was recognized as having been present twice with the same color, the differently shaped pictures."

Analysis by means of the various cues above described now proceeds pretty well, but the "Aufgabe" of full and complete recall of just what had been presented remains so dominant that all speculation is repressed. In the fifteenth sitting *Bef* is linked, apparently by associative abstraction, to its pattern but the cue is not followed up and no further gain is made.

P., XV, *Bef* (*Yol, Dut*). "This syllable *Bef* is associated with a certain visual image which appeared immediately. The shape of the visual image is not very distinct, but the pattern of the paper is. It is arranged in squares. This visual association came spontaneously and at once. The mind sought to force a verbal association without definite success. The only syllable that did come was *Bef Laj*, but there is no certainty that that is the one this evening. The association between *Bef* and the color of the paper is definite. There is somewhat of a dispute between two shapes for the visual image. One shape seems to be that of the parallel lines at the top and bottom with indented sides; the other the tall narrow picture. No definite certainty as to the shape, however, The syllable, *Laj*, did not seem to be associated with anything. It was the only syllable that appeared."

So near can one come to abstraction without reaching it. Sittings were unfortunately discontinued at this point.

Difficulties with the experimental conditions and consequent over-emphasis upon total recall prevented classification and, above all, speculation about the material, for this subject. This is the outstanding cause for his failure to reach an abstraction. Associative abstraction was in evidence, but its operation can be better studied in the protocols of S. and J., to the former of whom we now turn.

The attitude of this subject was marked throughout by certain uniformities. The first was the effort in the reaction period to reinstate the entire situation experienced with that syllable in the current presentation. Thus he endeavors to recall the associated syllable, the place in the presentation series, and the visual details of the picture. The subject's more immediate memory was thus very good, but he carries very little over from sitting to sitting, apparently because he makes no effort to do so. At any rate, things so remembered are neglected promptly—a very distinct form of abstraction, which, however, could not be studied.

A second feature of this subject's attitude towards the experiment was his effort in introspection to describe his experiences so far as possible in elementary terms, especially in terms of the kinaesthetic and organic sensations in which he does a great deal of thinking. Two examples from sitting V will make this clear:

"Promptly came creamy brown figure [drawn] with two smaller detailed figures on its face. It was accompanied by a general kinaesthesis, particularly of the upper part of the body, and a repetition of syllable

[illegible] and [illegible]

[illegible] figure vaguely [illegible] drawn. Mean-[illegible] connection [illegible]. It did not [illegible] be a con-[illegible] were vague [illegible].

[illegible]—for it is obvious [illegible] detail to be [illegible] from [illegible]. In the fifth [illegible] noticed the [illegible] and [illegible] a beginning [illegible] and one of [illegible] now undergone [illegible] been [illegible] beginning of [illegible] important [illegible]

[illegible]

[illegible] there was a [illegible]

[illegible] of the [illegible] of the [illegible] the subject [illegible] of memory. [illegible] the task [illegible] in the presentation period, the pattern being

identical in the first and third, the outline in the third and fifth. This position-framework is recognized to be the same no matter what figures are given, and hence is again very clearly an abstraction. Unfortunately we cannot trace its development, since in relation to the experiment it was largely accidental. We can only say that it was set up in the service of the memory attitude, which continued to rule, and that the mechanism was a deliberate trial-and-error method.

Before the nineteenth sitting, the subject was asked to "give the specific reference of each syllable". The subject retorted that he had been giving it to the best of his ability. In the first protocol he wrote as addition to the usual report: "The figure was one of the things 'meant' by the syllable—other things were position, exposure period, repetition, setting down introspections, accuracy of description, etc.—the whole group of acts surrounding the perception of the syllable."

It is small wonder that with all of this the subject had not reflected upon the experiment. It must be insisted, moreover, that he had deliberately tried to repress all speculation concerning the problem. When, just before sitting XXI, he was told to find the specific reference of each syllable to some detail or feature of the "picture," he objected that he had been prohibiting just such associations, but that the meanings of at least four syllables had become pretty clear. How he could not say.

From the protocols, however, it is quite obvious how this has come about, namely, by associative abstraction. This has been at work theoretically since the second sitting. Actually its influence has been clearly observable since the ninth. In associative abstraction a firm bond of association is established between the nonsense-word and the element with which it has always appeared, while the varying concomitants are connected less firmly. The difference may extend from a slight emphasis upon the constants together with clear recollection of the variables to a very great emphasis upon the constants together with absolute inability to recall the variables. Where the variables are limited in number and themselves objects of interest, as in these experiments, it more often happens that the constant associate of the nonsense-syllable is correctly recalled while with it appears, not the variable of that sitting's presentation, but that of some other, perhaps quite a long time before.

It is clear that associative abstraction has a great deal to do with the memory cues to analysis. The latter, however, are more comprehensive, including as they do separation of the features of the situation in memory by whatever means, and emphasizing the separation of the different features rather than the connection of one of them with the nonsense-syllable. The outlines were more often thus "abstracted" than the pattern;

and apparently, though not so certainly, color stands between the two. The *casual* operation of associative abstraction is one of its striking features. Nor could anything else be expected. For in addition to frequency, other factors influence association, more especially recency and intensity of impression. Thus it happens that, at the end of twenty sittings, the "specific meanings" of four only out of ten syllables were clear to this subject, while the meanings of all the syllables were clear to one subject who used what we may call the intuitive method at the end of three, to another at the end of seven sittings.

Very much the same remarks hold for subject J. as for subject S. True, she did not attempt, as the latter did, a fine introspective analysis of her reactions. But to write a protocol at all was an entirely new task for this subject (who had had no experience in experimental psychology) and absorbed her best efforts in much the same way as the analysis into elements had absorbed those of S. Like him, she adopted a memory "Aufgabe." Like him, she had begun an analysis of the situation into its elements and was tentatively associating, in a more or less accidental and external fashion, one nonsense-word with a certain element or group of elements when the warning was given in the sixth sitting against external and irrelevant assciations (see Appendix). And like S. she interpreted this as meaning that any association of a syllable with some one feature of the presentation was illicit. Her sole duty was hereafter felt to be to observe accurately, report fully, and not to speculate. This last task, she explained later, was by no means easy for her. Continually there came the desire to seek the underlying principle, but she always inhibited it. This is borne out by the protocols. A very striking characteristic of her procedure was her use of the two syllables as one name. It will be remembered that subject W. exhibited the same tendency, but abandoned it when she found that the syllables were used in varying combinations. J. does not thus give up the "name" idea. Each combination of syllables remains a particular name for a particular picture.

J., c XII, *Dut* (*Bef*). "*Bef Dut* visually appeared, then *Wej Dut*. No picture during reaction period. Afterward I saw pictures of both; of *Wej Dut* which is [drawn] with fine green stripes across it; then of *Bef Dut*, [also drawn] in the one square of which was the red butterfly. I realized my mistake made twice in reports of calling the last picture *Wej Dut* instead of *Bef Dut*, its true name."

This type of recall might in time have suggested, in a way that the subject could not inhibit, that *Dut* went with just the outline. Unfortunately, the operation of memory is not uniformly correct, and instances occur where *Dut* is said to go with some other shape. Yet by the end of the fourteenth sitting

some kind of system is really developing. Take, for example, the last protocol, where the reaction word was *Nud:*

"This brought up a visual image and almost immediately the syllables *Wej, Nud,* and *Jid.* I thought of the figure in brown [draws *Nud*] and then in blue, and wondered if I should ever be shown it in blue again."

Question: "When did the three syllables occur?"

Answer: "All came with the brown image."

Question: "Which seemed most intimately to mean your visual image?"

Answer: "Oh, *Nud,* then *Wej.*"[6]

From the fifteenth sitting on, associative abstraction becomes more and more effective. The shapes begin to be pretty correctly associated with their syllables, but the patterns are not yet names. Take, for instance, the subject's reaction to *Bef:*

XVI. "The picture named *Dut* appeared followed by the picture named *Yol.* [She draws both correctly with indications of a *Bef*-pattern]. In reaction period, I realized that both had in them this peculiar red figure and the same color of background."

Bef soon becomes associated with this pattern; but it should be noted that *Bef* is not thought of as a pattern but as a figure.

An excellent example of how far associative abstraction can carry one is seen in XVIII, where the reaction word was *Bef.*

"This syllable occurred to me as belonging to some picture having butterfly designs in it. It seemed to belong to *Nud* and *Yol* both."

At the end of the nineteenth sitting, the subject spontaneously offered the following remark:

"There is a tendency to associate pictures of one color or pattern, *e. g.,* all the pictures with lines every way in brown go together. Also the different pictures in blue with lines of two shades forming stripes. Both *Yol* and *Kuc* have had this brown pattern."

The experimenter determined to ascertain just how much the subject had learned. Accordingly the following questionnaire took place:

E. "What are *Yol* and *Kuc?*"

O. "*Yol* is the elephant and *Kuc* the picture with the cuts in the side." (Both right.)

E. "Can you name any others?"

O. "*Nud* is the tall picture with the rounded top." (Right.)

Questioned about the other figures, she replied that they seemed to change all the time. As remarked before, both *Kuc* and *Yol* had had this brown figure with lines.

E. "Do I understand you to mean that these are merely rather attributive of *Kuc* and *Yol?*"

The subject was puzzled and denied it. "I used to think that *Bef* was the name of the little red figure, but I don't think I can carry this idea through."

E. "What was the brown lines picture?"

O. "As to name?"

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[6]This answer must not be taken as proof of abstraction, as it is in accord with a characteristic set which somewhat hindered the subject's advancement, viz., her tendency to regard the shape as the really important thing, the color-pattern rather subsidiary. *Nud,* moreover, meant the whole picture, brown coloring and all.

Little additional light is thrown on the further development of the analytic process by the protocols of subject Mn. The analysis was fairly quickly and very definitely made. As has been said above, he also endeavored to reinstate the entire situation by the aid of any sort of mnemonic device. Some of these proved real aids to memory and all of them assisted in an analysis of the situation-complex. On the whole his reports are very detailed and his recall quite correct. In spite of a strong emphasis in the protocols on the visual image, it was evident that he remembered chiefly the verbal terms, either motor or auditory verbal or more probably mixed. He realized that the syllables had been changed around a good deal, and from the seventh sitting at least he realized that the pictures were compounds. But as his entire effort was to remember the presentation as seen with the given syllable that day, this fact is merely mentioned by him in passing.

Associative abstraction plays but a small part with this subject due to the episodic memory methods adopted. Hence, though the recall is fairly complete, there is little peculiar emphasis upon the constant concomitant. The subject was forced to break his sittings at the end of the fourteenth without having reached complete abstraction.

In summary, we find the subjects divided into two sharply differentiated groups according to their method of attack upon the experimental material. The first group had constantly in mind the problem of relevance, which was lacking to the consciousness of those in the second group. In the one case there was a definite intention to abstract, the purpose generally realized as such, to consider certain things only as relevant. In the other case, there was scarcely a trace of such intention.

The writer proposes that this intention be made the criterion of abstraction, and that only the former mode of attack be called abstraction. It is true that the term has been used in a more general sense for the process or processes by which one is more highly conscious of (or conscious only of) certain features of a complex situation. As thus used, it includes attention and what we have called (following Ach) associative abstraction. But this broader use of the term is not happy. For abstraction ceases to have, as immediate experience, any distinguishing characteristic. All of the part-processes described either in this study or in its predecessors are found in other contexts than abstraction. Abstraction in this broader sense would be distinguished from other processes only by the onlooker or by the reflective judgment of the observer. To use the psychologists' bug-a-boo, the distinction is "merely logical." If, however, we are allowed to restrict the term abstraction as proposed above,

a psychological criterion of abstraction as a process is given us in *the conscious intention to consider a given presentation in isolation from some or all of its relations.*

This intention may arise in the subject's consciousness in various ways: from instructions, as in the experiments of our predecessors in this field; from a knowledge of conditions and an acquiescence in them amounting to instructions, as with subject An.; from an analogy to previous experience, as with subjects W. and El. External conditions may favor in greater or less degree the arousal of such an intention; negative instructions can inhibit it to a very great degree; but with adults abstraction is such an habitual reaction-form of mind that negative instructions by themselves are not sufficient. The inhibition of abstraction where the material lends itself to abstraction (and most material does) implies not only very implicit acquiescence in instructions, but a consciousness busily engaged with other problems. With our subjects this intention was focal, but it seems likely that in most cases, after a short stay at the focus of consciousness, it recedes into the background.

The process of "true abstraction" as compared with "associative abstraction" is thus characterized by a difference of content, since in the latter the intention to abstract is lacking. The temporal course of the two is also different: true abstraction is rapid and sure; associative abstraction is in comparison slow and vacillating. Even in the end-result, where the two seem most alike, there is a marked difference. For in true abstraction, one may be distinctly conscious of the irrelevant, but conscious of it *as something rejected*; while in associative abstracting one is generally not conscious of the rejected material at all, or in lower degree. And this other content is not accompanied by a rejection-consciousness. Associative abstraction may appear as a cue leading up to true abstraction, while the reverse is never the case. These differences springing directly out of our experimental results leave no question, it seems to us, as to the inadvisability of considering under one heading two processes, psychologically so dissimilar.

This becomes even clearer when one considers the function of the two. Ach (1) has shown that associative abstraction is sufficient to establish general meanings.[7] A general meaning, however, is not the same thing as an abstract concept. Abstract concepts are called into existence in the service, immediately at least, of reflective thought. We form abstractions

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[7]Ach says that associative abstraction arouses a "Bewusstheit" of the general meaning, but the conclusion is the same even if one reject such creatures as "Bewusstheiten."

because we desire to consider a certain presentation or quality either isolated from all its relations, or potentially in all possible relations.

On the other hand, a general meaning, as the functional psychologists have insisted, exists in the service of practical needs. For this, the closer link given in associative abstraction between the class name and the invariable concomitant of the varying situations to which the name is applied may suffice. Associative abstraction may present to us, as meaning, the general characteristics only of a certain term, and hence may serve to call forth a generalized form of action. But for reflective thought this is not enough. Even though perceived in a welter of concrete particulars, the given quality must be recognized as the only one that counts. This recognition it is which associative abstraction cannot yield. Abstraction in the fullest sense is a process limited to those who have reached the level of purposeful thought; it arises chiefly by means of reflection and in the service of reflective thought.

Summary

Abstraction begins either with analysis or with conceptual assimilation of the presentation. These may be described as partial *versus* total assimilation, for analysis is found to involve conceptualization of the resulting analyzed parts. That which chiefly determines whether the assimilation shall be total or partial seems to be the presence or absence in consciousness of appropriate mental categories, which permit the total assimilation of the presentation without too great violence either to the categories or to the presentation. Yet even where the subject has clearly such a category, a mental "set" for analysis (whatever this may really consist of) may cause partial assimilation.

The process of analysis is initiated by certain cues. In the first group of these, there is a preferential selection, through the operation of attention and memory, of certain parts of the presented material. In a second group, analysis is initiated because of the essentially analytic means of communication; hence these may fairly be called social cues to analysis.

The third group is characterized by the deliberate and reflective intention to analyze. Deliberate analysis may be itself initiated or at least suggested by the other cues (hence we have clearly something of a cross-division), but this form of analysis is marked off from the other cases by the apparent activity of the self. Analysis may be initiated by several, not infrequently by all, of these cues acting in unison. Particularly noteworthy, however, is the great influence of language habits, an influence which permeates our commerce with our environment at every turn and which, by its very nature, inclines us to analysis.

Associative abstraction may suffice for the development of a general meaning, but something more is required for an abstraction. The essential element in abstraction is *a definite, though perhaps marginal, intention to consider a certain quality in isolation from any of its particular relations.* Only a mind which is capable of reflective thought has need of such an intention. The "mental set" which includes this intention may be called the abstractive attitude. This attitude seems to be variously aroused: by instructions, by more or less unmediated analogy to past experience, by reflective thought about the situation, by certain intellectual needs. It is this which must be added not only to associative abstraction but also to conceptualization and analysis in order to convert them into full-fledged processes of abstraction. As a final conclusion, one ventures to call renewed attention to the importance of the study of certain problems without giving the subjects definite instructions, not only to avoid the subjects' prejudices but also to avoid profoundly altering by those very instructions the nature of the process ostensibly studied.

Appendix

Before the sixth sitting (El.'s seventh), Group *c*, "the subjects were warned," to quote a note made at the time, "not to use irrelevant associations. For example if 'Bac' should be given, they were not to liken it to 'Baccy' and thence to something in the picture like tobacco. They were even to repress any such tendencies." It is regrettable that the exact wording was not preserved; for this instruction was critical for S. and J., perhaps for P. and Mn. They somehow understood the experimenter to mean that they were not to endeavor to connect any syllable with some aspect of the picture (whereas he meant only to prohibit mediate and external or accidental associations), and it was this which led to the peculiar form of their abstraction. In view of the importance of this change, even a contemporary note is unsatisfactory. At the same time, the following additional instructions were given verbatim:

"I want to make clear certain distinctions. When you see the syllable, it acts as a stimulus to certain experiences. These are, at least in large measure, terminated by your reaction. Certainly all *voluntary* search for an association should terminate, although there may arise *spontaneously* certain relevant experiences. It is to see if such be the case that a short pause is enjoined. Next you are to *retrospect* upon this experience just passed. You are not concerned now—at least primarily—with your present experience but with a past experience. You are to describe, as accurately as in you lies, what took place in your consciousness during the reaction time. If you add what came afterwards—as is proper, if the experience is relevant—make clear that it did come later: '*As I write the above,* I am conscious that it is entirely wrong.' Again, I want first of all your actual experience rather than your interpretation of your experience. I do not bar this latter—indeed I am glad to have it, but I want you sharply to distinguish between fact and interpretation. Moreover, there are two kinds of interpretation. There is the interpretation which comes to you during the reaction period and there is that which comes as you write. The former is *very* important, the latter is often quite useful."

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A NOTE ON WUNDT'S DOCTRINE OF CREATIVE SYNTHESIS

By E. B. Titchener

It was on a country-walk in the spring of 1858, Wundt informs us, that the notion of creative synthesis, which was to play so large a part in his psychological system, first occurred to him as the answer to the riddle of visual space-perception.[1] In formal outline, his doctrine remains the same from the *Beiträge* of 1862 to the *Physiologische Psychologie* of 1910. We all know, however, that between the *Vorlesungen* and the *Physiologische Psychologie* Wundt had radically changed his views; and the story of this change, in the context of space-perception, throws so clear a light on his psychological development and his systematic procedure that I think it worth the telling.

The Period of Unconscious Inferences

For the first account of the doctrine of creative synthesis we turn to the concluding essay of the *Beiträge*. Here Wundt analyses the process of perception into three unconscious acts of inductive reasoning: colligation, synthesis or fusion, and analogy. We consider them in order.

(*a*) The uniform connection of sensory stimulation with reflex movement means the repeated pairing and therefore the intimate connection of objective with subjective (muscular) sensations. The mind takes cognisance of this paired relationship by an unconscious act of colligation, a form of induction by simple enumeration. Since *A* has, a thousand times over, been followed immediately by *a*, and *B* by *b*, the mind argues that *A* will in the future always be attended by *a*, and *B* by *b*; or, in general, that an objective sensation will always be attended by a subjective sensation. We are still far removed from perception; the combinations *Aa*, *Bb*, leave the component sensations *A*, *a*, *B*, *b*, just what they were; but we have, in the act of colligation, taken the first step toward perception.[2]

[1] *Erlebtes und Erkanntes*, 1920, 181 ff. Cf. Ueber psychische Causalität, etc., *Phil. Stud.*, X., 1894, 123.

[2] *Beiträge zur Theorie der Sinneswahrnehmung*, 1862, 442. There is here, I think, a confusion, at any rate a verbal confusion, between colligation as external mode of connection (the paired sensations are *ipso facto* colligated) and colligation as unconscious act of generalization (the paired sensations are the material of an enumerative inference). I have read the passage in accordance with what I take to be its general sense.

(*b*) The essential act is that of synthesis. The mind cannot, however, advance to synthesis of its own accord. Something must happen, something in the nature of an accident, which disrupts the colligation and starts the mind enquiring and comparing.

Suppose, for instance, that there are two luminous points, at fixed distances from the eye, and that they set up two distinct retinal sensations. These objective sensations *A*, *B* are combined with their corresponding muscular sensations *a*, *b*; we have verified our colligation. But suppose further that the points shift to new positions while the eye remains at rest. We now have two objective sensations, *A*, *B*, with no attendant *a*, *b*; our colligation is proved wrong. What happens?—I translate the somewhat cryptic sentences in which Wundt describes the synthetic act.[8]

"In this way the two distinct retinal sensations are apprehended for themselves alone, and yet at the same time are compared with the muscular sensations that correspond with them. The connection with muscular sensations that colligation has established is broken, since the difference between the retinal sensations conditioned upon the changed position of the luminous points is apprehended for itself alone, and yet at the same time the change of position is measured by the degrees of the corresponding muscular sensations. Here begins the activity of synthesis. In isolating the retinal sensations, but at the same time measuring them by the standard borrowed from the muscular sense, it moulds perception to the form, the constraining impulse to which lies in the sense-impressions,—to the spatial form. Synthesis in perception is thus a creative activity, since it constructs space; but this creative activity is in no way arbitrary; the sensory impressions and the external accidents that play their part in synthesis make it altogether necessary that space be *re*constructed with absolute fidelity. The spatial form is the only form that can issue from the logical manipulation of the given elements of knowledge, and the spatial form is therefore the necessary product of this manipulation."

If I may be allowed to paraphrase these sentences in homely English, I think the course of the mind's argument would run somewhat as follows. The situation is, let me repeat, that two objective sensations *A* and *B* are given without their regular subjective accompaniments *a* and *b*. The mind then argues to itself, unconsciously, in this way:

"Here's an astonishing state of things,—a good *A* and a good *B* without a trace of *a* and *b*. How in the world can *a* and *b* have got away? Let me call them up, and see if anything has happened to them. Here's *a*,—it fits all right to *A*; here's *b*,—it fits all right to *B*. There's nothing the matter—curious, though, that *a* is so much stronger than *b*; I never noticed that before. Now I do think of it, I believe that all the local-qualities like *A* have strong *a*s, and all the local-qualities like *B* have weak *b*s. Yes, and I believe there's more than that; I believe that if the terms of the old familiar colligation were laid out I should find a real parallelism, graded series with graded series. But then there must be a reason, and I'm sure I don't—yes, but I do! I do see! I've got it! Why, if I only run *A* and *a* and *B* and *b* together, and all the rest in the same way, and just take every two

[8] *Ibid.*, 444.

in the lump, I can *place* *A* and *B*! I *am* placing them; *A* is there and *B* is *there*! Eureka! that's what that old colligation was for, only I hadn't sense to see it. What else *could* it have been for, graded local-quality on one side and graded strength of muscle-sensation on the other? The whole arrangement is luminous, once you know how to look at it. Now let me try the thing out elsewhere, and perhaps I'll find the rest of the world luminous too."

Great nonsense, of course. I submit only that it is more or less intelligible nonsense, that it reproduces the gist of Wundt's statement, and that it shows clearly the *locus* of the synthetic act. Not the nature of the associated elements, but the mind's unconscious logical *Verarbeitung* of those elements, is responsible for the emergence of the new type of experience. Left to themselves, under the external conditions of association, the elements would never have got beyond an habitual linkage that kept their character as experiences unchanged. The mind, as Wundt himself puts it later on, is thus like a logically-trained man of science, before whom the facts of colligation are laid with the request that he explain them.[4] It is true that, when synthesis has done its work, the resultant perception is exhibited to us as something natural and necessary, the only resultant that could have been expected. But just as there never was an unconscious mind whose operations did not reflect the conscious ingenuity of its inventor, so was there never a logically-trained man of science who did not, in the flashing moment of explanatory insight, take his explanation to be final and inevitable.

(*c*) The office of the third unconscious act, analogy, has been anticipated in the concluding sentence of my paraphrase. Analogy saves trouble and time: the work of synthesis, once done, need not be repeated. The act of analogy is therefore not essential, though without it we should find the business of perception laborious, and might have been satisfied to stop short of the refinements which we have actually achieved.[5]

The popular account in the *Vorlesungen* tells us that colligation is a generalising inference, which takes account only of externals; from the fact that a connection has taken place very often, we infer that it will always take place. Synthesis, on the other hand, searches for the ground of connection. It compares a number of colligations of the same sort, and traces the connective thread that runs through them; the connection then appears as necessary, and is therewith transformed into a fusion of the elements of knowledge. Synthesis thus furnishes something new, namely, the law of connection; it is the creative activity in the process of knowledge. The text then proceeds:

"The analysis which we have given [of space-perception] divides almost of itself into the two stages of colligation and synthesis. A luminous point appears in our field of vision; it arouses a retinal sensation of de-

[4] Ueber die Entstehung räumlicher Gesichtswahrnehmungen, *Philos. Monatshefte*, iii., 1869, 232; reprinted as Das Raumproblem in erkenntnistheoretischer Beleuchtung (1867)—a mistake for 1869—in *Kleine Schriften*, iii., 1921, 406 f.

[5] *Beiträge*, 444 f.

connected a movement-sensation,
with the distance of the stimu-
are formed in large
connected with a move-
a number of colligations
necessarily follows. Comparison
the movement-sensations cor-
the retinal sensations.
that quantitative rela-
the movements and which—
to the visual sensation
extensive relation. The
comparison, and the
and the product
over against
analogy steps in

the point more
this elaborate
to which
of logic
principle with
particular, makes
There is an ob-
and again as we re-
will send
would have
answer to the
perception
develop
The
possibility of
manages
exhibit the

"instruments and activities" whereby a perception of space arises. The instruments are the two systems of local signs, the one a system of different qualities, the other a system of graded intensities. The activities consist in the interplay of these two systems. "A measure of the spatial distance between any two points can be obtained only by means of an intensively graded system of signs; but a system of qualitatively different signs is required in order that the metric relation obtained by means of the first system may be transferred to the qualitative impressions and in this transference be made permanent. The efficiency of either system thus necessarily depends upon the efficiency of the other." Beyond this point, Wundt says, psychology cannot go. The uniform connection of qualitative local signs and feelings of innervation "is" the perception of space. "And if my critics object that the question of the construction of space out of intensive sensations is, after all, no more answered by my theory than by others, I have nothing to reply. That question, if it is directed to psychology as empirical science, has been wrongly addressed."[7]

It looks, then, as if psychology were coming to its rights. No doubt, the perceptive theory which Wundt is championing presupposes "the familiar psychical processes of association and reproduction, of unconscious judgment and inference;"[8] but he has been able, nevertheless, to set it up free of logical scaffolding; and the reader who mistrusted the unconscious mind might even now, with sufficient psychological faith, have taken the theory and left the logic. It is, however, plain that Wundt is merely resting, for the moment, at a half-way house. The only plausibility that his theory can boast—and we have seen that, to our modern eyes, it is little enough—derives from the creative act of synthesis. Our supposed reader, for all his psychological good-will, must have been content to argue that, since intensive sensations are the sole possible basis of space-perception, any theory is plausible which assumes a strictly genetic form.[9] Wundt would hardly be satisfied to make so much virtue out of so bare a necessity. He must, therefore, unless that logical synthesis is to be restored, go further on his psychological way; he must seek a new plausibility, and must raise the question how it is at all possible that intensive sensations be translated into a spatial form. This question he takes up, two years

[7]Neuere Leistungen auf dem Gebiete der physiologischen Psychologie, *Vjs. f. Psychiatrie*, i., 1867, 33, 36, 40, 45, 46. Among the hypotheses dropped is that of the origination of eye-movements in reflexes (38).—Cf. *Philos. Monatshefte*, iii., 1869, 225; *Kleine Schriften*, iii., 1921, 399.

[8]*Vjs. f. Psychiatrie*, i., 1867, 45 f.

[9]Cf. Wundt's criticism of the empiristic theory: *Vjs.*, 39 f., 44. This criticism is renewed in *Philos. Monatshefte*, iii., 1869, 225 ff.; *Kleine Schriften*, iii., 1921, 399 ff.

later, in a detailed study of the genesis of visual space-perception.[10]

Space and our conscious contents have after all, Wundt reminds us, certain characters in common. They may both alike be subsumed to the concept of magnitude; they are both continua, magnitudes whose progression is continuous; and they are both continua of manifold dimensions. Here, however, the resemblance ceases. For the dimensions of a continuum may be variously interrelated. They may be disparate, which means that, though they belong to one and the same magnitude, they are otherwise wholly independent of one another and can never pass over into one another. They may be homologous, which means that they not only are themselves capable of continuous progression but also permit of continuous passage from one to another; colors, for example, represent a continuum of two homologous dimensions. And lastly they may be congruent, which means that they are not only homologous but also interchangeable; any given section of one dimension is congruent with an equal section of another. It is plain, now, that space is a manifold continuum of three congruent dimensions, and that our ideational content is a manifold continuum of three disparate dimensions—quality, intensity, time. Hence the question before us runs: How may a continuum of three congruent dimensions be derived from a continuum of three disparate dimensions?

The answer turns on the nature of these disparates. We can form a continuum of x congruent dimensions if we have given (a) a continuum of x homologous dimensions, to serve as material of measurement, and (b) a continuum of one (disparate) dimension, physiologically connected with it, to serve as measuring scale. By applying scale to material for all possible distances and in all possible directions we render congruent the homologous dimensions of the material and we read x dimensions into the originally one-dimensional scale. The procedure may be described either as the reduction of the heterogeneous material to measurements of a homogeneous scale, or as the multiplication of the homogeneous scale by means of a heterogeneous material. Its result must be the formation of a continuum of congruent dimensions.

Colors, as we have seen, represent a continuum of two homologous dimensions. The system of qualitative local signs is evidently a continuum of the same kind, and thus furnishes

[10]See Note 4. The question, after what has been said in the *Vjs.*, can hardly fall within the limits of empirical psychology. In his introductory paragraph Wundt proposes to discuss it "von einer allgemeineren psychologischen Grundlage aus;" but the new title of the *Kleine Schriften* makes it epistemological. The argument is utilized both in the *P. P.*, 1874, 685 f., and in the *Logik*, i., 1880, 459.

us with a material of measurement. In searching for a scale, we have to choose between the two one-dimensional continua—the two remaining disparate dimensions of the total ideational continuum, time and intensity. We can hardly hesitate. Every measurement by a time-scale would depend, first, upon the distance between the terminal points within the qualitative continuum which constituted the limits of the interval to be measured and, secondly, upon the velocity with which this distance had been traversed. The units of the time-scale would thus be variable and incomparable. So we are left with intensity, the intensity of movement-sensations or sensations of innervation; and here we find, in fact, both constancy of metric result when scale is laid upon material, and also that physiological connection between the two factors in measurement which the successful solution of our problem demands. A space of two congruent dimensions is thus assured.

What, then, of the third spatial dimension? We must, obviously, retain our intensive scale; for all three dimensions of space are congruent. But if logic and experiment are at one upon this point, so are they also at one as regards the original material of measurement, which can be nothing else than the third one-dimensional constituent of ideation, namely, time. Time-order in ideation, succession, plays a far larger part in the perception of depth than in the perception of surface, and the perception of depth itself shows always a certain indefiniteness, which points to a more or less variable and unreliable material. Presently, it is true, the work of time may be supplemented by that of other and more dependable materials, such as the stereoscopic differences between the images of the resting retinas. But unless time had been there to play its part as original material of measurement, we should hardly have attained to the perception of the third spatial dimension.

"Now, therefore, we can understand how it comes about that our ideation, which represents primarily a continuum of three disparate dimensions, is able to develop, in space, a continuum of three congruent dimensions." Wundt has answered his new question, and in answering it has once more made his theory plausible.[11]

The Psychological Period

We may still ask, however, whether this novel plausibility is more than logical. Does the essay of 1869 really do anything else than replace the unconscious induction of 1862 by a process of conscious deduction? And is the theory of visual space-perception, as a psychological theory, itself bettered by the change?

[11] *Philos. Monatshefte*, iii., 1869, 238 ff.; *Kleine Schriften*, iii., 1921, 413 ff.

We have, fortunately, Wundt's own reply to these objections in the doctrine of the *Physiologische Psychologie.* Five years are still to elapse before that work appears. When it comes, we see that Wundt's thought has profited both by the negative and by the positive results of the articles we have been considering; it has become through and through psychological. On the title-page of the *Beiträge* he could set the Leibnizian motto: *Nihil est in intellectu quod non prius fuerit in sensu—nisi intellectus ipse*; and the *intellectus ipse* shows as the unconscious mind, always ready to explain by inductive argument what otherwise must remain inexplicable. On the title-page of the *Physiologische Psychologie* he might (save for the dog-Latin of it) have set the motto: *Nihil est in intellectu quod non prius fuerit in sensu—nisi processus quidam penitus psychologicus.* The theory of space-perception is henceforth a psychological theory, wrought out in terms of immanent psychological process.[12]

We are all familiar with the details, and I need not spend time on them. The uniform teaching of the *Physiologische Psychologie* is that the "idea of space issues in every case from the connection of a qualitative manifold of peripheral sensations with the qualitatively uniform feelings of innervation, which by their intensive gradation are suited to serve as a general measure of magnitude," and that this issuance is psychologically conditioned.[13] The form of the statement changes, of course, but its substance remains. We have all learned it, and we are all prepared to reject it. Let us not forget, however, the tremendous array of experimental evidence that Wundt was able to set forth in favor of his hypothesis; let us not forget either the conceptual background against which he worked, and in particular the vague evolutionism of the late sixties and the seventies, which would make no bones of deriving space from the non-spatial. The synthetic theory was boldly planned and conscientiously wrought. It not only marked, scientifically and critically, a vast improvement on the older views, but for many

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[12] As early as 1869 we find Wundt saying that "our consciousness does not originally possess the idea of space, but forms that idea by way of a *psychological process*" (italics mine): *Philos. Monatshefte*, 238; *Kleine Schriften*, iii., 413.

[13] The sentence quoted stands in 1874, 641; ii., 1880, 177 (sensations of innervation); ii., 1887, 207 (sensations of movement); ii., 1893, 233. In the first three editions Wundt's theory is genetic and synthetic; in the fourth it is a genetic theory of associative fusion. In the fifth and sixth editions the theory is a preempiristic and genetic theory of complex local signs, and the sentence changes to: "Only the conjunction of 'retinal image' and 'movement image' can produce the actual image of the object" (ii., 1902, 686; ii., 1910, 736). The identity of doctrine is affirmed in ii., 1910, 736, note 1.—The difference between the view of 1869 and that of 1874 is shown in a couple of sentences, 1874, 629 f.

years it also stood *facile princeps* among its contemporaries. And even now we, who have passed beyond it, cannot replace the Wundtian doctrine by anything as solid, as comprehensive, as unitary; the great single problem has split up into part-problems, and for these there is still a sad lack of phenomenological observation.[14]

I shall say no more, then, of the theory of creative synthesis. I wish rather, in conclusion, to call attention to certain corollaries to the preceding discussion,—points of unequal importance, but all illustrative of Wundt's personal 'psychology.'

(1) In the first place, we now seem to have a fairly clear picture of the genesis of the *Physiologische Psychologie*. We know the studies and researches upon which Wundt based his section on the physiological properties of the nervous system. We know that he had materials in the *Vorlesungen* for the section on sensations; and for the *Physiologische Psychologie* he could make use of Helmholtz' *Tonempfindungen* as well as of the *Optik*. We know, from what has been said in this paper, how seriously he had busied himself, in the interval between the two books, with the psychological problem of perception. We know, as I have elsewhere shown, that the doctrine of apperception shaped itself while the *Physiologische Psychologie* was actually preparing.[15] We know of his study of historical and current systems of psychology. We know, finally, of his physiologically motived interest in the reflexes, and of his social-psychological interest in expressive movements: in 1874 he had the use, too, of Darwin's *Expression of the Emotions*. All in all, therefore, we possess the materials for a detailed study of the sources and composition of Wundt's greatest book; and we may hope that some one of his pupils will presently undertake the task.

(2) It is natural to compare Wundt's doctrine of creative synthesis in perception with the 'mental chemistry' of the associationists.[16] There is, of course, no reference to mental chemistry in the *Beiträge* or the *Vorlesungen*. For one thing, unconscious logic was there making the requisite synthesis; chem-

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[14] This breaking-up of the perceptive problem has meant also, of course, a shift of perspective or point of view. See esp. O. Külpe, *Grundriss d. Psych.*, 1893, 349.

[15] This JOURNAL, xxxii., 1921, 596 f.

[16] In *A History of the Association Psychology* (1921, 179, 264 f., 289 f., 293 f., etc.) H. C. Warren has emphasised the importance of this concept to associationism. I hope to return to the subject in a later Note.

istry was not needed.[17] For another thing, the space which results from the logical act is still, on the face of it, a natural and necessary resultant; nothing else could have been expected; whereas it is of the essence, *e. g.*, of J. S. Mill's mental chemistry that the unexpected happens; who should anticipate the issuance of white from the prismatic colors? We may accordingly find it the more significant that references to Mill occur in all six editions of the *Physiologische Psychologie.*[18] There could be no better proof, in a matter of detail, of Wundt's intention to psychologise his theory of perception.

(3) Lastly, I offer this paper as an illustration of one of Wundt's salient traits,—his profound respect for the continuity of his own thinking. As we pass from the *Vorlesungen* to the *Physiologische Psychologie*, we enter a new world. Wundt has discarded all the cumber of unconscious inference and can attack the preceptive problem psychologically. But he does not start afresh: that is the characteristic point of our whole story: he rather holds fast to the concept of creative synthesis, and seeks only to psychologise it. This temperamental trait, the tendency to retain his original conceptual tools long after they have done the work for which he forged them, runs through all his systematic work. It explains why there are two distinct theories of creative synthesis; it explains how he could pass as easily as he did from a sheerly motor sensation of innervation to "memory-images of movement-sensations;"[19] it explains why his doctrine of apperception represents not one but several theories.

[17]Wundt was familiar with the work of the British associationists, and also in all likelihood with the *Beytrag zur Physiologie der Sinne* of J.G. Steinbuch (1811). He tells us in 1869 (*Kleine Schriften*, iii., 407) that he deliberately turned his back upon associationism in order to make his experiment in logicism.

[18]See 1874, 639 f.; ii., 1880, 175 f.; ii., 1887, 205; ii., 1893, 231; ii., 1902 684; ii., 1910, 734. It is odd to find Wundt, in the last two editions, turning the tables on Mill by the remark that Mill's mental chemistry fails to lay due stress on the 'creative character of this psychical synthesis!'

[19]Cf., *e. g.*, *Vorlesungen*, i., 1863, 221 f. (or *Vjs.*, i., 1867, 47) with Zur Lehre von den Gemüthsbewegungen, *Philos. Studien*, vi., 1891, 387 f.

SYNAESTHESIA AND MEANING

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1. Introduction

Although a vast amount of literature on synaesthesia has accumulated over a period of time as long as a century and a half, the real root of the problem seems never to have been unearthed,[1] and the difficulty has been that no investigator has attacked the problem intensively from an introspective point of view.

The investigation herein reported aims to describe the functioning of synaesthetic phenomena in a blind subject and to offer as check data the results from similar experimentation upon a second blind subject who is asynaesthetic. The data were obtained at different intervals of time from 1916-1921, but always under the same conditions and with carefully guarded instructions. Our synaesthetic subject, Thomas D. Cutsforth (*A*), is a graduate student in psychology and a well trained introspector. He lost his sight at the age of eleven. Our other blind subject (*B*), Leslie C. Blades, is likewise a well trained introspector, and at the time of experimentation was an advanced student in psychology. He lost his sight at the age of nine.

[1]See R. H. Wheeler, The Synaesthesia of a Blind Subject, *Univ. of Ore. Pubs.*, i, No. 5, 1920.

Owing to the fact that during part of the investigation observer *B* was not available, data from a third subject (*W*), the senior writer, have been used as check material.

The present study falls into two parts: (1) an analysis of the process of recognizing, and (2) a study of the development of meaning. The instructions to the reagents will serve as a description of method.

2. Group 1. Experiments on Recognition

Instructions to B.—"I shall present to you a Braille letter, punched in the middle of a rectangular piece of tag-board. You will find the tag-board located on the table directly in front of you and in a small wooden frame. I shall give you a ready signal, followed by 'now', whereupon you are to inspect the letter in normal fashion as if reading. The instant you have recognized the letter, stop and begin your introspection at once. Ready, now."

(a) *Typical Introspective Data*

1. *Observer B.* "(The letter was 'l'.) The *Aufgabe* was present to consciousness in terms of a focal perception of *E*'s voice, together with incipient tendencies for the fingers of my right hand to assume the position of investigating a point-letter and the vocal-motor-auditory: 'Wonder what it will be.' As I began to make the arm and finger movements across the tag-board from left to right consciousness was occupied with tactual sensations of smoothness and kinaesthetic sensations of keeping my finger in the proper position; these latter were localized principally about the wrist and elbow. During this procedure, however, my attention was centered tactually upon the tip of my second finger and I was conscious of a tactual 'set' or 'anticipation', a preparedness to receive an actual tactual impression of a point, part of which was also incipient strain localized in the finger and knuckle joints. Along with this 'set' were tensions about the brows and shoulders. Then I was tactually aware of touching the first point, the outstanding feature of which at the outset was a momentary consciousness of something other than smooth surface; this consciousness did not develop into a full-fledged awareness of a distinct point, but turned at once into an awareness of a solid line running at right angles to my finger movement. All of this occurred while the right side of my finger was in contact with the points and while the left-hand side of the finger was still resting upon smooth surface; in the fringe of consciousness there still lingered an awareness of this smooth surface. There then developed a very fleeting and vague tactual image of the letter 'l'; I noticed the difference between this tactual image and the perception which had thus far developed; the perception had as yet no definite ends either at the top or the bottom; it was merely the perception of a line; but the image contained a definite height; that is, the distinctive features of the perception were its straightness and direction while the distinctive feature of the image was its vertical length. I then found myself hesitating momentarily—a suspension of judgment—consisting merely of a slight tendency to inhibit arm and finger movement and of rising tensions in my throat and shoulders. I then moved my finger until the points fell beneath the middle of the finger-tip; this movement was not straight to the right but was made in the form of a tiny arc; thereupon the letter clarified, first by a momentary standing out in focal tactual consciousness of the top point, the lower two points re-

maining as a bar or solid line. While the upper dot was thus being focalised I was momentarily conscious of its roundness and smallness. There then reappeared the tactual image of 'l', which entered consciousness very suddenly and developed at once to a high degree of clearness. I then found myself saying 'l' and began to relax, generally. During the entire process I was not conscious of familiarity as such. In fact the instant my tactual image of the 'l' appeared I began to relax."

2. *Observer B*. [Similar instructions, except that a New York Point letter was used.] "(The letter was New York Point 'f'.) As before, I was first aware of tactual and kinaesthetic sensations having to do with approaching the points from the left. The first point appeared as a localized, blunt pressure, with attention momentarily taking in the roundness and smallness of the point, while tapering off in consciousness was the smoothness of the paper against the left half of the ventral surface of my finger-tip. Then as my finger covered the letter as a whole I did not perceive the individual points; first there appeared a tactual bar or line with no definite beginning or ending; as this 'line'-consciousness lingered for an instant the three points became distinct, arranged in a straight line. Then there appeared a vague tactual image of a short line—its shortness and direction being the only definite features—in response to which I found myself relaxing and at the same time having the vocal-motor-auditory: 'f'."

Instructions: "I am about to present you with an object which you are to recognize tactual fashion. It will be located in the center of a rectangular piece of tag-board and the tag-board will be in a cardboard frame. Place your finger here (*E* guided *A*'s hand) opposite and to the left of the object. When I say 'ready' approach it slowly and as soon as you have recognized it begin to introspect."

3. *Observer A*. "As I laid my finger upon the tag-board and began to move across toward the right I was conscious of wondering what the object might be........, of the smoothness of the paper, and of kinaesthetic sensations in fingers, wrist and elbow. Then my attention was claimed, tactually, by a localized and rather sudden welling-up of a pressure sensation which did not develop into a consciousness of a Braille point but merely into the awareness of something blunt. The instant the above tactual impressions appeared I began to visualize the yellow of the tag-board; but when I came in contact with the first point my attention was absorbed with the change from smoothness to bluntness. Before this consciousness developed very far I became aware of two more blunt points localized adjacent to the first one; here attention centered momentarily upon the mid-position between the latter two points. There then at once appeared visual imagery of a jeweled pin, only the head of which stood out focally in consciousness; it was localized at my finger-tip and was followed very suddenly by incipient finger movement upward and to the left as if to ascertain if the pin would roll when I moved it. This visual image was fleeting and vague, involving merely a small area of yellow with two bright spots in the center. There then developed an awareness that the jewels might be set in the form of a cross, whereupon I found myself making very slight and incipient finger movements of exploring for the other jewels. In this process my finger came in contact with a point above those which I had already perceived; the instant this point appeared I found my attention grouping the three left-hand points spatially; the three-ness of the points gave way to a tactual line in which direction and length stood out as its prominent features. During this time the yellow and white of the visualized jeweled pin had been persisting. But now the tactual perception enlarged to take in the fourth point, on the right of the figure, and

at this instant the jeweled pin vanished, and suddenly taking its place was a yellowish-red blotch localized at my finger-tip; momentarily the tactual processes entirely receded from consciousness and I found my attention entirely absorbed with this patch of color, which was stable, clear and vivid. Then I found myself tending to relax; muscles loosened about the fingers, wrist, arm and shoulders, together with tendencies to take a long breath. This relaxation might have been interpreted as a feeling of familiarity, but I was not aware of it at the time as such. The appearance of the yellowish-red was my identification of the letter as 'k'. I had no vocal-motor imagery or other identifying imagery until after I knew that it was 'k'; the color informed me that the letter was 'k'."

4. *Observer A*. (Same instructions. *A* was aware that the object would be a Braille letter.) "The fore-period consisted of visual-tactual and visual-motor processes having to do with anticipating Braille letters. On beginning to move my hand to the right and slightly upward along the paper, as is my custom when reading point letters, I was first conscious of tactual-kinaesthetic-visual smoothness of the paper and of arm-movement. Upon reaching the first point I received a blunt tactual sensation together with visual imagery of the point raised up like a tiny knob from the surface of the tag-board and colored the yellow of tag-board. The second point was perceived and visualized in a similar fashion. But I had no sooner perceived the second dot than the two became spatially arranged in an oblique line slanting down and to the right. There followed a very sudden recognition of the letter as 'o', which consisted of a sudden change from the yellow-buff tag-board color in which I had been visualizing the points into a very light, faded potato-peeling-grey—an almost colorless, watery grey—tinged with a faint bluish darkness. But no sooner had this latter visual process developed than it was interrupted in its course by a tactual perception of another point above those which I had already detected. Before this latter tactual consciousness had grown into a definite tactual-visual awareness of a point I found myself in kinaesthetic fashion tending to make the bend formed by the three points; this was a fleeting and vague image but was localized in my fingers and wrist. Immediately following was the sudden appearance of a small bluish black area localized at my finger-tip, the visual image tending, as it developed, to assume the shape of the figure formed by the three points. Here my attention was centered upon the darkness, hue and shape of the visual process and the tactual elements entirely receded from consciousness. The color appeared suddenly and with no warning, the immediate antecedent of which was the motor image just described; it seemed as if the visual image instead of tactual processes were coming from my finger-tip; so sudden was the advent into consciousness of this visual image that it had developed before my finger had fairly covered the last point in the letter; immediately upon the advent of this visual image I relaxed. During the false recognition of the letter as 'o', tensions had increased; I had been momentarily conscious that the *Aufgabe* had not been fulfilled; this had consisted of facial, neck and shoulder tensions. Then (and now) I was not conscious of familiarity as such; the suddenness with which the color appeared, its persistence in focal consciousness with subsequent bodily relaxations, are all that I could say constituted familiarity. But my relaxation means to me a fulfillment of the task rather than an awareness of familiarity. The letter was 'b'; the bluish-black visual image meant 'b'."

5. *Observer A*. (Same instructions. The letter was 'q'.) "This process of recognizing was extremely rapid and involved only one movement of my finger. As before, I was at first conscious of the smoothness of the paper and of finger movement followed by the sharply localized blunt pressure of the lower point. I obtained the next two points as a bar, not two isolated points, with the focus of attention centered upon the mid-

space between them. This tactual-spatial perception was very fleeting and vague, and accompanied by a relatively much clearer visualization of the points in yellow-buff, raised upon the tag-board. With no intervening processes this visual imagery at once shifted to the reddish brown of 'f'; there followed a very attenuated perception of the upper two points of the figure, arranged as they came, in the form of a bar; this tactual-spatial perception lingered just long enough for me to 'catch' it and then shifted suddenly into the dark, pale, bottle-green of 'q'; the five points at no time became spatially arranged as a group either tactually or visually; the bluish-green of the 'q' appeared from above, rapidly covering the reddish-brown of the 'f', just as a curtain might fall and cover the one behind it. I had no sign of verbal imagery; the color of the 'q' covered a larger area on my finger-tip than did the color for 'f', the former being a larger letter. I was able to detect the manner in which the first color, meaning 'f', entered consciousness: it first appeared just above the upper joint at my finger tip and spread rapidly over the previously visualized points, covering the lower one last. In case of the 'q' the color appeared at the top, as did the 'f', first blotting out the upper edge of the colored 'f'-patch; it then spread downward until it formed a square area; then the left side of the square continued to 'grow' in a downward direction, forming a one-sided extension or appendage on the square. Thus the color, as it developed, shaped itself in the general form of the letter; it was only in terms of this shape of the visual image that I was conscious of the spatial arrangement of the points which go to make up the letter."

(b) *Summary of Introspective Data on Recognition*

Observer A. A's procedure in recognizing Braille letters may be summarized as follows. He first perceives the point or points with which he comes in contact. If the first point happens to be isolated, the tactual sensation momentarily lingers in consciousness; but if he comes in contact with two or more points simultaneously, he does not perceive these points as individuals, but finds that his attention is grouping them into a linear bar. For a brief instant this spatialization is tactual-visual, but tends at once to give way, entirely, to visual imagery of yellow points raised upon yellow tag-board. If the letter be a simple or small one, including not more than two or three points, there at once appears a colored area localized at his finger-tip. This area 'grows' into the shape of the letter, and the particular hue or brightness of this visual image constitutes either a tentative or final labelling of the letter. If, however, the letter is a large one, involving four or five points, this first color to develop may be a false recognition. The correction is made in terms of a new or second color which "drowns out," "covers," or "blots out" the preceding color. This last color to appear, the color which finally identifies the letter, is aroused by an antecedent consciousness the duration of which is so brief that it is reported merely as the awareness of "striking against something." The undeveloped consciousness of an additional point or bar of points in the figure serves to usher into focal attention the appropriate and final color. This undevel-

oped consciousness does not contain identifiable tactual elements. It is merely consciousness of "something."

Feelings of familiarity consist merely of the suddenness with which a color develops at the finger-tip, and the centering of attention upon the hue and shape of the colored area, with now and then a kinaesthetic process of anticipation or verification.

A's processes of recognizing, then, consist not essentially in the arousal of feelings of familiarity but in the *behavior* of colored visual imagery. If a color appears and runs its course with no interruption by the subsequent arousal of another color, the recognition is complete; if one color is superseded by another, the first constituted but a tentative or false recognition. In either case it is a color which has identified the letter in question.

(c) *Recognitions of A and B Compared*

B, like *A*, found himself arranging the points into bars or lines the instant they were detected tactual-motor fashion, and like *A* he was not aware of individual points as such unless they occupied an isolated position in the letter. In *B*'s case, as in *A*'s, the first spatial grouping of the points is vague, and does not include definite spatial limits; lines or bars are perceived as straight or as running in a certain direction; but at first their exact length or height is not perceived. In both reagents this vague tactual perception undergoes a process of clarification. *B*'s procedure here involves the appearance of a tactual image of the letter in which the bars or lines possess definite dimensions. In *A*'s case this clarification-process takes place in terms of visual imagery. Continued finger movement in both reagents results in prolonging and in further definitizing this spatial grouping of the points. In *B* the grouping is done in tactual-kinaesthetic terms; in *A* it is done in visual-kinaesthetic terms, the visual feature of the process beginning with images of the yellow-buff tag-board points spatially grouped as they are visualized, and ending in another colored visual image in which the individual points have lost their identity. This latter visual image identifies the letter by means of its hue and shape, principally by hue. This final grouping of the points in *B*'s case undergoes a similar dropping out of concrete and identifiable points; the spatial blend becomes perfect—the individual points lose their identity and appear as a grouped formation. This group is perceived as triangular, square, or what not, according to its form. But *B*'s mental content in this procedure is implicitly tactual, while *A*'s mental content is visual, the previous tactual processes receding or disappearing altogether.

To sum up *B*'s procedure in recognizing: there first appears a shift of attention from individual points to bars or lines; then

from bars or lines either to tactual imagery of a letter or to more definitely perceived spatial arrangements of these points. Here the points lose their individuality as separate bluntnesses and fuse into a spatialized blend; this latter process gives way to definite tactual imagery or to vocal-motor imagery of the letter. There is no visual imagery during the entire process.

Thus *B*'s processes of recognizing consist of the development of tactual and later of verbal imagery from tactual perceptions. *A*'s processes of recognizing consist of the development of visual imagery from tactual perceptions. The content which labels the letter in *B*'s case may be a tactual or a verbal image. In *A*'s case it is invariably a colored visual image of a certain hue, brightness or shape. These colors are identical with those which appear whenever *A* is thinking in terms of letters; they are the same as appear upon hearing the letters of the alphabet pronounced, and they are also the same as appear in his alphabet and number forms. Imagery of letters is invariably visual imagery of these colors and not tactual imagery.

The striking fact which is derived from a comparison of *A*'s procedure with *B*'s is the similarity in function in the two individuals but the marked difference in mental content. *A* recognizes letters by means of their synaesthetic visual associates, but the fact that *A* is synaesthetic does not render his processes of recognizing functionally different from *B*'s. Stage by stage the two individuals recognize in identical fashion. Both reagents must identify the object to be recognized; in either case the object must be labelled. *A*'s synaesthetic visual imagery is not an incidental and superfluous process in recognizing; there are no incidental or superfluous processes in *B*'s recognitions. *A*'s colored associates for letters identify or label those letters, as does *B*'s tactual or verbal imagery.

(*d*) *Significance of A's Synaesthetic Phenomena in the Process of Recognizing*

The temporal position of *A*'s synaesthetic visual images in processes of recognizing clearly points to but one interpretation, namely, that the visual associates are the essential components of the process and that they are integral parts of the development of meaning. The blurred tactual perception of points in the case of recognizing Braille letters, for example, means nothing to *A* except as a preliminary stage in the course of recognizing, for it must be definitized by visual imagery. Then, after the points have been perceived in their spatial arrangement through the aid of visual imagery, the object does not yet mean a certain letter rather than any other letter of the alphabet. This visualized grouping of points must undergo a change or be identified with the aid of some subsequent process. It hap-

pens to be a stereotyped colored visual image and hence a synaesthetic image which thus identifies a certain spatially arranged group of points as 'b,' or 'l,' or 'q,' or what not. A parallel procedure is to be found in *B*'s processes of recognizing; but the latter involve tactual and kinaesthetic contents in the place of *A*'s synaesthetic images. Where meaning develops in *B*'s case by means of rapid attention-shifts from tactual perception to tactual image or from tactual perception to verbal image, or from tactual perception direct to motor relaxation, it develops in *A*'s case by means of a rapid shift of attention from an undeveloped tactual perception to a visual grouping of points, and hence by way of a stereotyped visual synaesthetic image to motor response, or (in the more mechanized recognitions) directly from an undeveloped tactual perception through a synaesthetic visual image to motor response. In every instance, however, a motor response or relaxation will not label a letter; there must first appear a colored visual associate. There is no doubt, therefore, that the colored associates of letters *mean* those letters, in *A*'s case, and that without these colored associates the process of recognizing could not take place.

This fact gives us a clue to the functional significance of familiarity in processes of recognizing. If we compare *A*'s procedure with introspections obtained by Woods in her investigations of recognizing[2] we discover that *A*'s colored visual imagery functions as a substitute for the incipient organic and motor responses which her *O*s reported as feelings of familiarity. The shift from tactual to visual attention in *A*'s case means familiarity; colored visual associates function both as a general and as a specific label for the object recognized. We assume that a feeling of familiarity is but a species of development of meaning, and that in a general way it identifies the object to be recognized as "old" or as "belonging to past experience." Since in *A*'s case the color which appears in connection with a certain perception is always the same as the color which appeared with that perception in all previous experiences, the presence of the color itself in a subsequent experience *is* the familiarity of that experience. In other words, since the visual synaesthetic image is always stereotyped, it *is* the same experience which was had before. There can be no doubt about the identity or sameness of the experience. The invariable and stereotyped feature of the visual associate thus attenuates the process of "feeling familiar;" it provides implicit certainty in any subsequent recognition-process; it allows for no hesitation, wondering, or fluctuation of attention. Hence the relatively prolonged and laborious process of experiencing a feeling of familiarity is unnecessary

[2] This JOURNAL, 26, 1915, 313-387.

in *A*'s case; for an object or perception indefinitely labelled as familiar is by the same process labelled once and for all definitely and concretely. The perception is not only "old" or "familiar," but a definitely and finally identified perception at the same time.

When we ordinarily recognize as familiar an object, a face,or a name, we often have the experience of "knowing what the object is" or of "knowing the name of the person whose face is familiar," but we are unable to recall the appropriate name which will finally identify that object or person. *A* experiences a certain difficulty in recognizing which further demonstrates our point made in the previous paragraph and which stands out in contrast to the difficulty ordinarily experienced by an asynaesthetic person. *A* often has the difficulty of properly translating a colored visual associate into the appropriate name. He has already definitely labelled a person or object by means of his own color-language, but he has forgotten the name which translates that color into the English language. Thus, in teaching biology, he sometimes recognizes and as far as his own consciousness is concerned he has definitely and finally labelled a certain butterfly or crustacean. The colored visual image which stands for that species is present in his consciousness, but the technical name by which the species is generally known has escaped him. In such instances as these technical names, as such, are entirely superfluous to *A*'s processes of recognizing or of labelling. They mean to him only terms by which the identity of the species in question is made known to other people. We have come to use these technical names themselves as final contents which function in our own private processes of recognizing as well as contents which function in social communication. But not so for *A*, whose own private processes of recognizing are complete when the appropriate colored visual associate appears, regardless of the subsequent arousal of a technical name. We believe that this illustration shows further how synaesthetic phenomena not only serve as substitutes for other imaginal processes in acts of recognizing, but also function as substitutes for "feelings of familiarity." Synaesthetic phenomena function both as general and as specific labels for objects to be recognized.

We cannot understand how all this could take place unless synaesthetic phenomena are derived from, and are directly allied with, synaesthesia proper. When a person has synaesthesia, for example colored hearing, a certain blue means a flute tone just as much as the auditory quality itself, and probably more so. As far as meaning is concerned, that person sees the flute tone as much as if not more than he hears it. The color has come to function as an integral element in the process of perceiving. And so with the more prolonged and elaborate processes of perceiving which we call recognition.

A further comparative illustration will show how *A*'s processes of perceiving are not different functionally from those of asynaesthetic persons. When the senior writer hears a flute tone he finds that the focal standing-out of the mellow, woody, round-like quality, together with tendencies to visualize a flute or to say "flute," constitute the *meaning* of flute. It is the behavior of attention with respect to the auditory, visual and kinaesthetic qualities which are present in the act of perceiving which constitutes the development of meaning. And without tendencies to visualize or to say "flute" the tone would not be that of a flute any more than that of some other similarly sounding instrument. But let one sound a flute in *A*'s presence: the mental contents which appear are different, but they function as has just been described. *A* does not attend to the fluty qualities as such at all; in fact he hardly hears them. But something rises suddenly into focal consciousness to take the place of focalized auditory qualities, for no sooner is the flute sounded than a certain quality of blue arises to focal attention. So far *A*'s procedure corresponds exactly to the procedure of an asynaesthetic person. In the former, a visual quality has developed; in the latter, an ordinary quality has developed. Next, however, in *A*'s case, the hue or other qualitative feature of the visual image is recognized or identified. This particular hue or quality identifies the sound as coming from a flute. This procedure corresponds exactly to that of an asynaesthetic person, who has tended to visualize a flute or to say "flute," in that both persons have now by their final procedure recognized or labelled the flute tone. As long as *A*'s attention fails to take in the blue color in perceiving a flute, he does not know the sound of a flute from the roaring of a distant locomotive whistle. So long as a similarly functioning contextual image does not develop in the consciousness of an asynaesthetic person, the sound of a flute cannot be differentiated from any other sound. We conclude that in the very beginning of *A*'s mental life synaesthesia developed as an integral part of perceiving. His synaesthesia *is* the act of perceiving, itself.

A's synaesthetic images function in like manner in his processes of recognizing. While from the point of view of the asynaesthetic person *A*'s visual associates stand for something else and mean something else than visual images, we must not conclude that their functioning in consciousness is different from that of the asynaesthetic individual. In so concluding we forget that the process of deriving meaning even in perceiving and in recognizing demands the presence of some process other than the one recognized or identified. Certain contextual imagery is necessary, such as the visual or the vocal-motor mentioned above in connection with perceiving a flute tone. *A*'s synaesthetic images happen to constitute this contextual imagery.

3. Group 2. Auditory Method

The above experiments lead to the conclusion that synaesthesia is a phenomenon of meaning in *A*'s case. In order to determine whether our interpretation and findings were correct, a second series of experiments was performed on subject *A*, with *W* as check observer.

In this series of experiments various lists of nonsense-syllables were presented to the reagent in auditory fashion. Each list included one or more meaningful words having the same number of letters as the nonsense-syllables, and was so arranged as to make the process of introspecting upon the development of meaning as easy as possible. This was done by varying only one letter of the syllable each time, as for example: bih, bij, bik, bil (l); or fab, mab, sab, pab, lab (oratory); and the like. Thus *A*'s consciousness during the presentation of the series and up to the appearance of the meaningful word would not radically change and would be simple enough in content to make a very detailed introspection possible. The changes which would then appear, as soon as *A* became aware of a meaningful word, would stand out in contrast to the previous mental processes.

The instructions to *A* were as follows. "I am going to present to you a list of nonsense-syllables, auditory fashion, in which there will appear, sooner or later, a meaningful word. The instant you are aware of meaning respond by saying 'now'. I will then stop the presentations. You will be asked to give me a very detailed introspective description of the processes involved in the development of the meaningful process." The syllables were presented at 1 sec. intervals in order to prevent the arousal of associations and in order possibly to provide instances in which *E* might repeat a meaningful word without arousing a response on *A*'s part.

(*a*) *Introspective Data*

6. *Observer A*. Series: bih, bij, bil (l). Response at once to the word 'bil'. "As *E* was repeating the first two syllables I was at no time focally aware of the sound of his voice; each time, just as the sound commenced, I was aware only of 'something', non-focally; then I found that my attention was at once claimed by a dark, bluish, thick amorphous patch of color, about the size of one's hand; with this color I was aware, non-focally, of repeating the syllable in vocal-motor imagery; the development of processes following the presentation of the third syllable was very rapid. Just as *E* was saying the 'b' of 'bil (l)' there appeared the same blue patch as before, and as before I found myself vocalizing the syllable, but with the focus of attention centered upon the patch of color. In case of the preceding syllables the blue patch remained during the vocalization of the syllables, *i. e.*, the color did not change. But I no sooner found myself vocalizing the word 'bil (l)' than there appeared, extended off from the right side of the blue patch, an area of white, from the 'l' sound in 'bil (l)'; the color came in as if the blue patch grew outward toward the right, while the left side

of the patch retained its original blue; along with the appearance of the white I found my eye-movement tending toward the right with the growth of the visual image. No sooner had this extension of color appeared than I found myself responding in motor fashion; and as I was reacting I had faint visual imagery of a sheet of paper which was the beginning of a consciousness that the 'bill' meant to me a legislative bill; the immediate antecedent of my response was the extension of the blue patch; meaning appeared with this behavior of the visual image, and this meaning was subsequently made more concrete by the appearance of the visualized sheet of paper, which, in turn, was interpreted in terms of vocal-motor imagery, but after I had reacted. I forgot to mention that with the appearance of vocal-motor imagery of 'bil (l)' the blue part of the synaesthetic image increased slightly in size and became more saturated."

7. *Observer A*. Series: bop, bor, bov, boy. Response at once upon hearing the word 'boy'. "As before, the auditory perceptions of the first three syllables involved a shift of attention to visual patches, the color of which was the same blue as for the previous syllables, and which was determined by the 'b' sound in the syllables. As I perceived the 'b' of 'boy' there appeared the same blue patch; then as my attention lingered upon this visual image I found myself saying the word 'boy' in vocal-motor imagery, but only non-focally; here the qualities of the vocal-motor image are indistinct and vague, obscure and undefinitized; it would be impossible to describe its qualities because there are no distinguishable qualities present other than a general image of movement, localized in my throat. In exactly the same fashion as in the previous case, the blue patch then extended on the right, first into the light yellow of the 'o', and farther on into a brighter yellow of the 'y'; my attention was wholly absorbed in these color-changes, which seemed to develop rapidly of themselves. I then found myself responding."

8. *Observer A*. Series: cag, caj, caz, cat. "During this experiment I found that the color of the first two syllables was determined by the 'ca' sound of *E*'s voice. I set up for myself the subsidiary task of inhibiting my vocal-motor imagery in order to ascertain whether or not I was using it. As a result of this inhibition of vocal-motor imagery I was unable to perceive the 'cag'. The 'ca' sound aroused a smoky-blue white, but I was unable to tell whether or not it had any meaning. The color merely appeared and did not change. Just as *E* was repeating the second syllable I had vocal-motor imagery of the first syllable—the 'cag' which I had only imperfectly perceived before—whereupon I recognized the colors as colors for definite letters; my recognition consisted wholly of a rapid shift from vocal-motor to visual synaesthetic imagery. I had previously recognized that the colors for 'ca' stood for 'ca' by means of accompanying vocalization of the 'ca'-sound; this color had drowned out the color for the 'g'; in previous experiments I found either that the color for the last letter in the syllable was not entirely drowned out or that I vivified that color by vocalizing the last letter; when *E* said 'cat' I was already anticipating the smoky-white patch which stood for the 'ca'-sound; but no sooner was the word pronounced than I had vocal-motor imagery of the syllable together with focal visual attention centered upon an extension of the smoky-white patch toward the right into the reddish-brown of the 't'; I then found myself reacting."

9. *Observer A*. Series: vad, yad, zad, sad. "When *E* repeated the first syllable, I found my attention at once claimed by a patch of colors, the hue of which was determined by the letter 'v'; on hearing the second syllable this color was slightly changed by the difference in the sound of the first letter, 'y'; then I at once began to anticipate the appearance of the third syllable in terms of this same color; so that, just as the third syllable was repeated, my visual attention was already absorbed with this color;

but the instant the 'zad' was pronounced there appeared a conflict between the persisting yellowish-grey of the 'y' and a reddish-brown patch, which latter tended to appear with the auditory perception of the 'z' sound in 'zad'. The first stage of this conflict consisted of a 'shaking' or 'shivering' of the 'y' color; then the colors for both 'y' and 'z' began to smudge or mix together into a sort of 'emulsion' of color, in which spots and streaks of one color intermingled, spatially, with spots and streaks of the other color; then I found myself having vocal-motor imagery of 'zad', whereupon all trace of the yellowish-grey of the 'y' vanished and was replaced by a smooth patch or area of reddish-brown. This awareness of color-conflicts was accompanied by a developing motor attitude of dissatisfaction and unpleasantness, characterized by tendencies to frown, marked tensions in the throat, and tendencies to tighten about the chest and shoulders. But there then tended to appear the yellowish-grey patch of the 'y' accompanied by vocal-motor imagery of 'y'; this was but the beginning of a consciousness that the first letter of the syllables should begin with 'y'; I was trying to visualize all of the first letters of the syllables alike instead of the last letters; this reappearance of the color for the 'y' took place just to the left of the persisting reddish-brown of the 'z', and for a moment there was perceptible eye-strain of tending to look toward the right as my imaginal line of regard lingered in the direction of the 'z' color. By this time *E* had pronounced the syllable 'sad'; this happened just as my visual attention was beginning to shift for the last time from the reddish-brown of the 'z' to the yellowish-grey of the 'y'; the auditory perception of 'sad' was accompanied by a sudden appearance of a light yellow patch which at once superimposed itself on or 'slapped down' over the persisting greyer yellow of the 'y'; the next instant I found myself having vocal-motor imagery of 'sad'; and together with this verbal image the light yellow patch enlarged toward the right, changing into a dull greenish blue, indicating the 'd' sound of 'sad'; concomitantly with this shift or extension from the right-hand side of the yellow patch I was aware of a motor attitude of satisfaction, consisting of pleasantness, which was bound up with chest relaxations, of relaxations about the abdomen, throat and mouth. This response also served as an awareness that the task was over. During the entire process, however, my attention was focussed upon changing colors and brightnesses."

10. *Observer A*. Series: hov, hot, hom, hop, hok. The reagent did not react until *E* came to the syllable 'hok', when he responded at once. "Upon perceiving the first syllable there appeared a very definite and vivid chocolate-brown color representing the 'ho'-sound, so vivid that I found myself momentarily absorbed in gazing at the color as it persisted in the focus of my visual attention. Then, as I found myself having vocal-motor imagery of the syllable, meaning tended to develop. This consisted first of a tendency for the chocolate-brown color to extend on the right into a very light, yellowish white; then it seemed as if I should subsequently recognize the meaning, but I could not; this latter consciousness was a slight tendency to relax in the presence of the changing color. But the motor relaxation—about chest and face—had but just begun when it ceased to develop and I found my attention centered, visually, upon a persisting yellowish-white patch; I was gazing blankly at the extended colored patch. This gazing continued until the next syllable was pronounced. The brown of the 'ho'-sound then considerably brightened and became more saturated; my visual attention lingered on this color more focally than before, but still nothing happened; nothing new then happened until *E* pronounced the word 'hok'; up to this time I was merely having the synaesthetic imagery *plus* the verbal imagery of the syllables; upon hearing 'hok' I vocalized it as usual, but there at once developed on the right-hand edge of the chocolate-brown patch a reddish-brown, almost orange extension; I found myself responding; the word had meaning the minute

this change in the visual imagery took place. The response consisted of a general, diffused bodily tendency to relax, particularly about the throat and chest. This response seemed to me a recognition of the fact that the change in the visual image had meaning. I then said 'now'."

11. *Observer A*. Series: xap, wap, sap. "During the fore-period I was aware of increasing strains about the throat, chest and jaws. Upon perceiving the first syllable, the appropriate color appeared at once in the foreground of my visual field. Nothing new appeared in consciousness until *E* repeated the word 'sap'; at this juncture I was already repeating the synaesthetic image in anticipation of the coming syllable; as I vocalized this syllable there appeared to the left of the 'p'-color the light yellow of the 's'-sound. Together with this change in the visual imagery I found myself tending to relax, but this did not develop very far. Consciousness was claimed focally by tendencies to fixate my visual field more rigidly than before; I was moving my line of regard about the yellowish-white synaesthetic image of 'sap' which was still persisting; this was the beginning of a consciousness of wondering what 'sap' it was; then the colors in my visual field turned into a large green area, out from the bottom of which there developed a brownish form; this was the beginning of a consciousness that 'sap' referred to 'tree sap'; then a tree-trunk stood out, developing from the brownish form which had just appeared; and as my visual line of regard centered itself upon the tree-trunk I visualized beneath the bark a layer of white wood. This completed my consciousness that the 'sap' was 'tree sap'. During this time I was non-focally aware of greater and greater bodily tensions, located particularly in the chest and abdomen. These latter processes consisted of a tendency to inhibit the response until the meaning was complete. The meaning seemed to develop in the changing visual imagery, and my motor attitude seemed to constitute a recognition of the fact that meaning was developing."

12. *Observer W*. Series: qec, pav, mus, yix, log. (Owing to *W*'s tendency to anticipate meaning in series of syllables arranged as for observer *A*, a different type of series had to be presented him.) "First I was aware of tensions about the neck, in the throat and shoulders, and about the chest, having to do with a rigid bodily attitude of preparedness to react quickly as soon as a meaningful word might appear. As *E* (Mr. Cutsforth) read the syllables I found myself attending focally to each one in turn, tending at the same time to translate a momentary focal auditory perception of the syllables into visual imagery of the syllables printed in large black letters on a white background; immediately following each syllable my attention was claimed by the rapid fashion in which all auditory features dropped out of consciousness, and by a shift to consciousness of my bodily attitude which was just described; in the auditory perceptions the consonants tended to stand out far more prominently than the vowels; yet these sounds disappeared from consciousness almost as quickly as they appeared. When the word 'log' was given I found that my auditory attention developed to a higher state of vividness and more vigorously than in case of the previous syllables; the perception of the auditory qualities was more sudden; there seemed to be a different quality in the auditory perception itself, consisting of the dominance now of the vowel-sound instead of the consonant-sounds as before; while this auditory quality of the 'o' sound developed suddenly to a very high degree of clearness, I found myself already responding bodily. This bodily response consisted of an incipient tendency to pull my shoulders together, to bring my head forward, and to jerk slightly my entire right arm. While this motor reaction was going on, my attention was at first still centered upon the 'o' sound of 'log'. But no sooner had the 'o' sound developed to a maximal degree of clearness than I tended to visualize, dimly, in a background of white light, the word 'log'; this process did not develop very far, however; during

this time I was looking in the direction of *E*, and slightly to the right of my line of regard there appeared but the beginning stage of a visual image of a timber log, lying on its side. Merely the shape of one end of the log, faint suggestions of corrugations of bark, together with a dark brown color appeared with any distinctness at all. Accompanying this shift in visual imagery from the word 'log' to a visualized log the motor reaction which had already begun greatly increased in intensity and became a generalized motor 'set' in the direction of the visualized log. The entire reaction was so rapid and the motor processes so diffuse and widespread that I could not detect many of the kinaesthetic details; the more sharply localized tensions were in the throat and about the eyes; the diffused parts of the reaction seemed to be vaguely localized in chest and abdomen. (I think that if someone had asked me, at the time I was perceiving the word itself, whether or not it was meaningful I should have said 'yes', although at that time there was no process which seemed to tell me what that meaning was. But upon the appearance of the visualized log the meaning became definitized. This particular visual image was so fleeting and vague that I am surprised that it occupied the focus of attention. I was attending to it rather than to the motor background.) The motor background seemed to consist of an attitude of recognition which I assumed toward the visual image of the log; it seemed to consist, in other words, of a recognition of the fact that the processes had meaning."

(*b*) *Summary of Introspective Data on the Development of Meaning*

So far as our series of experiments is concerned, development of meaning consisted, in *A*'s case, (1) in the manner in which visual synaesthetic phenomena developed; and (2) in the arousal of a diffused motor and organic reaction which took place immediately following the development of the visual synaesthesia. The first stage has to do with the growth of meaning proper, and the second has to do with the reagent's recognition of the fact that meaning has developed or was beginning to develop.

In perceiving a nonsense-syllable, *A*'s attention shifts to a visual concomitant of the sound of the syllable even while *E* is pronouncing it. This visual associate is determined by the dominant or repeated sound in the series of syllables. As long as in the perceiving of these syllables no processes take place other than a vocal-motor image of the syllable, together with the visual concomitant, the reagent does not react; the syllable is meaningless. The verbal image appears with the aroused visual associate, and is sometimes repeated in order to clarify or prolong the visual associate itself, but the verbal process remains non-focal throughout. When a meaningful word is pronounced by *E*, *A*'s procedure of perceiving the word begins as do his perceptions of meaningless syllables; his attention at once shifts from auditory qualities to their visual associates. But from this point on his procedure is different; meaning begins to develop. The antecedent of this development of meaning consists of a non-focal verbal image of the word itself,

together with the persisting visual concomitant, which latter is the same for the verbal image as it was for the original non-focal auditory perception. Then the visual associate which is being attended-to focally extends, or grows in size, taking on additional coloration as it does so. These colors are the synaesthetic associates of the remaining letters in the meaningful syllable.

So far the development of meaning has passed through its first stage, but the process is not complete for the reason that as yet the meaning itself has not been recognized. The instructions were to react when meaning developed. Hence the reagent was disposed to recognize meaning itself before he reacted for the benefit of *E*. If, after these colors changed as just described, *A* did not find himself assuming a motor attitude, *i. e.*, responding bodily to the change, the process of meaning was halted in its course. Such an attitude included relaxations in the chest, throat, and sometimes abdomen, and organic disturbances which were interpreted to be pleasantness. Without such an attitude or without some further mental process *A* finds himself unable to interpret the change in the color. The change in the color means something, but what? Vocal-motor imagery will not come to *A*'s aid, for the vocal-motor imagery itself is non-focal and cannot be attended-to as such; moreover the change in the visual synaesthetic image which has already taken place has identified the verbal image so far as it can be identified. Thus, with no further mental processes other than a shift in the colors, *A* finds himself staring at them, blankly; the colors themselves are unfamiliar or meaningless until they, in turn, have been identified.

If, on the other hand, with this change or extension in the visual synaesthetic imagery, *A* finds himself reacting in organic-motor fashion as well, he responds to the syllable as meaningful.

So far in our analysis of the development of meaning we have traced its course through two stages—that of coming into existence and that of being recognized afterwards. But our development of meaning is yet incomplete. The meaning is still general. Sometimes *A* reacted when the meaning was still in this generalized form (introspections 7-10); he reacted before the concrete meaning had been defined by behavior of additional mental contents. Sometimes he went on and defined this meaning.

Thus, if there is in *A*'s behavior an implied *Aufgabe* to go on and define this meaning (see introspections 6 and 11) there takes place a third stage. A second change appears in his synaesthetic phenomena. The synaesthetic processes already present to consciousness give way to further visual imagery or to com-

bined verbal and visual imagery. These latter processes define the meaning of the word, specifically. For example, in one instance *A*'s visual attention shifted from synaesthetic imagery of the word 'bil (l)' to visual imagery of a white sheet of paper. This was *A*'s method of defining 'bill' as a legislative bill. Again, the yellowish-white synaesthetic image of 'sap' gave way to a rapidly developing visual image of a green tree, hence to a tree-trunk in which white wood was seen beneath the bark. This was *A*'s procedure in limiting or defining the general meaning of 'sap' which had already developed when the synaesthetic image of 'sap' appeared. During the development of this third stage in the growth of meaning the organic reaction, characteristic of the second stage, is prolonged and this reaction functions as a general motor background for the subsequent mental contents.

W's procedure is not different functionally from *A*'s, but with respect to mental content it is quite different. *W*'s attention is suddenly and focally claimed by the auditory qualities of the nonsense-syllables. But when a meaningful word is pronounceds, this auditory perception develops with greater vigor and to a higher degree of focality. This suddenness and ease with which auditory qualities stand out in consciousness corresponds to the sudden clarifying and extension of colors in *A*'s consciousness of meaning. In either case the meaning has not yet been recognized. The procedure of both reagents thus far constitutes an implicit recognition that the word is meaningful, but the meaning itself has not yet been recognized. If the development of meaning should stop here, *W* would have found that the word "seemed familiar" or was "about to turn into meaning" or "ought to have had meaning." Such an experience on *A*'s part may be found in introspection 10. From this point on the appearance of visual imagery in *W*'s case and the shift from visual synaesthetic imagery to further visual imagery in *A*'s case constitute final limitations or definitions of the meaning which has already tended to develop. A motor-organic reaction in either case constitutes a recognition of the meaning itself. The additional visual imagery (it may not always be visual in *W*'s case) is the stimulus which evokes a widespread motor attitude, intensifying the already existing attitude, and is also the content which defines the meaning of the word. The prolonged motor attitude, which was at first a recognition of general meaning, now becomes a recognition of defined or limited meaning.

Thus meaning develops as a dual process consisting (1) of shifting mental contents which determine the presence or absence of meaning and whether the meaning shall be general or specific—a stimulus function; and (2) of the development of a

motor attitude in the presence of these shifting contents which constitutes a recognition of this meaning—a response function. The contents function as stimuli and the motor attitudes as responses. If the first appear without the latter, meaning is not complete. The *O* is then conscious only that meaning began to develop but failed of recognition. Thus the full growth of meaning consists not only of the process-aspect of shifting mental contents, but also of motor responses in the presence of these process-aspects.

There exist the same temporal relations between the focal standing-out of auditory perceptions and the motor response of recognition in *W*'s case as exist between the appearance of a synaesthetic image and the motor response in *A*'s case. *W* testified that meaning began to develop when the auditory processes become highly focalized, and that it was his subsequent consciousness of a motor attitude, which seemed to constitute a recognition of this developing meaning, that enabled him to offer this testimony. *A* testified that meaning began to develop when his synaesthetic image "extended," and that it was his consciousness of a motor attitude, immediately afterwards, that enabled him to offer his testimony. Careful reviews of a mass of introspective data verify these interpretations. We believe, therefore, that synaesthetic phenomena and meaning are species of the same genus of mental functions. Synaesthesis is an integral part of every cognitive process in a synaesthetic reagent.

A synaesthetic phenomenon is but a type of behavior of attention; and this type of behavior, whenever and wherever it takes place, constitutes a process-aspect of mental contents which in turn constitutes meaning.

We do not propose to assume that the growth of meaning takes place in all *O*s and at all times just as we have described it; but we are confident that, so far as our own experimental method is concerned, and so far as our own synaesthetic reagent is concerned, mental contents function as they have been interpreted.

(c) *Significance of Synaesthetic Phenomena in the Development of Meaning*

Our results from this series of experiments confirm results from earlier series. (1) Synaesthetic phenomena function in differentiating meaningless from meaningful processes, and are thus cognitive phenomena as far as their function in mental life is concerned; (2) these synaesthetic phenomena are identical with their original prototypes, synaesthesis proper; (3) in the same fashion as visual associates stand for auditory, tactual-kinaesthetic or other non-visual processes, these visual

associates stand for meaning; and (4) the functional problem in synaesthesia and in synaesthetic phenomena is the same; this functional problem is identical with the problem of meaning.

4. Group 3. Tactual Method

In order to demonstrate the development of meaning under such conditions as would necessitate a slow growth, several words were stamped in American Braille and exposed to *A*, one at a time, but in tachistoscopic fashion. That is, each word was exposed suddenly and rapidly several times, before the individual letters grouped themselves into a meaningful word. The reagent was instructed to make rapid sweeping movements over the word as a whole, at 3 sec. intervals, until the word was recognized. After a practice-series was performed in order that *A* might learn the necessary finger technique, a regular series of experiments was presented. Since the following introspection is typical of all our results we include only one example.

(*a*) *Typical Introspective Datum*

13. *Observer A*. Word: good. "(I recognized the meaning of the word upon the fifth exposure.) As the tip of my fingers passed over the word, the first time, I perceived the letter 'd'. This perception developed as follows: at the outset I was aware only of indefinitely grouped blunt points; these points at once became arranged, spatially, in terms of visual imagery, and at the same time took on the poorly saturated bluish-grey of the 'd'; at this juncture the obscure tactual qualities, which had at first appeared, entirely vanished and the color of the letter persisted alone in consciousness as my awareness of the letter itself. The second time I inspected the word the color of the 'd' persisted in consciousness; I failed to add any letters to my consciousness of the word; there only appeared meaningless and confused jumbles of tactual impressions which, as fast as they appeared, shifted to visual, grey imagery—my synaesthetic imagery of the temperature of the paper. But during this second inspection I anticipated in terms of grey-blue color the 'd' at the end of the word and also in terms of hurried eye-movement toward the right as my finger was moving across the word; this grey-blue visual image was a small irregular area of color localised at the right end of a rectangular grey form, which latter represented the word as a whole. The third presentation resulted in perceiving the 'g'. My consciousness of the 'g' developed in the same fashion as did the 'd'; the very light greenish white color for 'g' persisting in its appropriate position with respect to the oblong block. The fourth inspection resulted in perceiving the second 'o', which appeared first as a meaningless and indefinitely grouped mass of blunt points; the pressure qualities at once shifted to visualized points, and then suddenly into the dark, smudgy black of the letter 'o'. The last time I inspected the letters my attention was being claimed by persisting visual imagery of the three which had been perceived; I was only non-focally aware of finger movement; just as my fingers touched the points between the letter 'g' and the second 'o' (the points which were to become the first 'o'), and before these points were given time to arrange themselves into the spatial grouping of a letter, there appeared very suddenly a visual synaesthetic image of a double 'o'; this latter process was built up by an extension which grew out from the left side of the already existing black smudge which represented the second 'o'; that is, this already existing smudge doubled in size, forming a bar instead of a

small patch of darkness. But no sooner had this 'oo' developed in terms of visual imagery than the colors for the single letters (which had up to this time been separated by small inter-spaces of grey background) suddenly merged into a continuously colored streak. In this merged visual image, containing three colors, my attention was not centered upon any one particular color, but upon the group of colors as a whole. Together with this merging of the visual imagery I found myself having the non-focal vocal-motor image: 'good'. When I discovered the first 'o' I noticed the beginning of a motor expectation, consisting of incipient forward movements of my shoulders and of increased throat tensions. (I interpreted this motor attitude as meaning anticipation of fulfilling the *Aufgabe* and also desire to find out the meaning of the word.) As the colors of the word were merging into a solid group I found myself relaxing bodily. I distinctly noticed that the colors began merging just antecedent to the appearance of the vocal-motor 'good', and continued beyond my awareness of this verbal process. I will now describe the merging more in detail. First the color for the letter 'd' slightly shifted its position toward the left until its edges met and fused with the right boundary of the black smudge of the 'oo'; then before this change in position had completed itself the colored patch representing 'g' shifted slightly to the right until its edges fused with the left boundary of the 'oo' smudge. As these edges fused or blended, the colors now representing the word as a whole imperceptibly 'ran into' one another, very much, I suppose, as the colors of a spectrum. I was aware of the shiftings of color before I attended to my developing motor response. The motor attitude itself seemed to mean anticipation and expectancy on the one hand, and a fulfillment of the task on the other. The merging of the colors meant to me a recognition of the word, and this merging was emphasized by the vocal-motor imagery."

(*b*) *Summary and Interpretation of Introspective Data on Tactual Presentations*

Two problems present themselves with reference to the results of this and the former series of experiments. One consists of the significance of verbal imagery in connection with *A*'s synaesthetic phenomena, and the other has to do with the significance of *A*'s motor responses in the development of meaning.

The temporal relationships between the appearance of verbal imagery and of synaesthetic visual imagery in this and the preceding series of experiments indicate that verbal processes contribute to the development of meaning. A synaesthetic image is aroused either by an auditory or a tactual stimulus; but before this synaesthetic image has run its course, *O* finds himself having verbal imagery of the syllable or of the word. Since the verbal process *is* the syllable or *is* the word, one would expect that ordinarily no further process would be necessary for the development of meaning. The verbal image ought to identify or to label the auditory or the tactual perception. In *A*'s case, however, the verbal image always remains non-focal. But invariably the presence of such an image is directly followed by a change in the synaesthetic image toward greater focality—the visual image becomes brighter, more saturated, more permanent. Hence the verbal image seems to

be the cue by which the dissociated synaesthetic image is definitized. Further evidence of this function of verbal imagery is found in instances in which a synaesthetic image persists, alone, after the original auditory or tactual stimulus has faded entirely from consciousness. In the absence of such a non-focal process as the persisting but vague auditory or tactual feature of the stimulus word, the persisting and now detached synaesthetic image *means* nothing. This fact suggests that not only is meaning the process-aspect of changing mental contents, but that the content toward which the shift is being made must begin to develop in the presence of the dwindling of the content from which the shift was made. And when a synaesthetic image lingers in consciousness beyond the duration of the stimulus-content which aroused it, it becomes meaningless the instant the non-focal stimulus-content entirely vanishes. Under these conditions *A*'s verbal imagery appears as a substitute for the original stimulus-content; this introduces a new process-aspect, a new shift of attention, now involving a verbal image as the content away from which attention shifts; and the persisting synaesthetic image is the content to which attention shifts. Thus a re-shifting of attention toward the synaesthetic content provides it with the lost meaning.

The question arises: does the verbal content provide meaning to the persistent synaesthetic image, or does the existing synaesthetic image now function to provide meaning to the verbal image, as it did to the original auditory or tactual stimulus? Since the synaesthetic image existed before the verbal image came in, we assume that the verbal process identified the visual (note, the visual, momentarily, had no meaning prior to the appearance of the verbal process); but the verbal process, in *A*'s consciousness, has no meaning until identified by a visual synaesthetic image; hence, upon the appearance of the verbal process, the persisting visual process which lasts longer than the verbal subsequently defines the verbal process itself. This accounts for the fact, perhaps, that the visual synaesthetic image becomes intensified or clarified upon the appearance of the verbal process. It not only was identified but it, in turn, identified the process which had just identified it. This interchangeableness of stimulus and response functions, or of cause and effect, is not inconsistent. Logically, causes and effects and stimuli and responses must be interchangeable to function at all.

As to the second problem—the significance of *A*'s bodily motor responses in the development of meaning—our data from the present series of experiments furnish us with important suggestions. A careful study of *W*'s introspections in contrast to *A*'s shows that the former reagent lays much more stress upon kinaesthetic processes in the development of meaning

than does *A*. We have explained that these kinaesthetic processes evidently functioned as contents of recognizing the meaning of a word or syllable as it began to develop. The meaning was the "object" recognized, and the motor response was the recognition of the "object." But *A* showed fewer motor tendencies than did *W*. Evidently, then, something aside from motor tendencies in *A*'s case sufficed in the recognition of meaning, once that meaning began to develop. We interpret this difference between *W* and *A* to mean that in the former reagent processes of recognizing meaning are more explicit or overt, while in the latter reagent these processes of recognizing are more implicit; that is, they are to be assumed in the behavior of his synaesthetic imagery. We have already seen how in *A*'s case synaesthetic imagery labels an antecedent mental content both in a general and in a specific manner, thus attenuating "feelings of familiarity." This fact in itself means that *A*'s recognitions of meaning tend to take place implicitly because of his synaesthetic phenomena.

It is evident, however, that as far as meaning is concerned colors and their behavior alone do not suffice when a given train of associations terminates in a synaesthetic image. To possess meaning, *A*'s synaesthetic colors must themselves be recognized, or they must function as if they were recognized. One procedure by which these colors are recognized when they occur in prolonged sequences is by a shift of attention from one color to another. The product of this shift defines the antecedent color, implicitly. But if a train of visual associations fails to lead to a further shift in visual imagery, the development or the course of meaning is halted. Here the reagent must resort either to verbal or to other kinaesthetic processes. The final color is then recognized by means of a motor attitude. Thus, as long as colors keep changing from one to another, meaning is present for the time being, and whether or not such colors shall suffice to produce meaning depends upon whether or not the behavior of colors fulfils either an implicit or an explicit *Aufgabe*. Thus, in our last experiment, colors sufficed not in themselves but because they united the letters of the word "good" into the word "good." This union of colors satisfied the *Aufgabe*. In introspection 13 the behavior of colors did not satisfy the *Aufgabe*, for it led neither to a further change in colors nor to a bodily relaxation. Hence the colors meant nothing. To return, then, to a comparison of *W* and *A*, *W* habitually fulfils *Aufgaben* by resorting to complex motor attitudes along with changing visual, auditory, and other imagery; *A* can fulfill the same tasks by juggling visual contents, and with less kinaesthesis. But the instant visual synaesthetic contents cease to flow, *A*'s motor responses come at once to the rescue.

We can conclude, therefore, that the behavior of synaesthetic phenomena in our synaesthetic reagent is an adequate substitute in focal consciousness for the motor responses which were more important in our check reagent. But our synaesthetic reagent must have motor responses constantly on tap in a case of emergency. A careful examination of his introspective data shows that such motor responses are constantly on tap as a dim and oftentimes unnoticed background. He attends to changes in synaesthetic phenomena as long as they run their course; but if these synaesthetic images fail to run their course, he becomes conscious at once of the failure of the motor background to run its course as well. *A*'s mental life is characterized by a synaesthetic tendency to 'see' everything. The motor background which develops with the growth of meaning, and which came to the front so many times in *W*'s introspections, is not noticed by *A* for the same reason that auditory or tactual processes are not noticed. Visual surrogates symbolize them all. So *A* is over-determined or predisposed to find meaning in his synaesthetic images rather than in motor adjustments, in the same fashion as he is over-determined or predisposed to "see" sounds rather than to "hear" them. But just as auditory qualities in his colored hearing determine the behavior of the visual associate, and operate as a necessary background, so in the development of meaning the motor attitude is a necessary background for the behavior of detached synaesthetic images.

It is because the reagent is attending to *visual* rather than to auditory qualities when he hears sounds that it seems as if he were "seeing" sounds rather than "hearing" them. It is because his attention is centered upon visual qualities when he calls up *auditory* imagery of a sound that makes it seem to the reagent that he has auditory imagery when in reality no auditory qualities are present. And because his attention is centered upon shifting visual contents as meaning develops, rather than centered upon motor contents, he finds meaning in the former rather than in the latter. In every instance the process attended-to is the one which the reagent interprets as the bearer of the meaning. In every instance the last process attended-to—the last to run its course—is interpreted to be the content of the process of recognizing the preceding content. Hence *A* finds meanings in motor attitudes only when visual accompaniments cease or when a motor response terminates a given series of experiences.

All of this is illustrated by introspection 13. While the asynaesthetic reagent would probably have interpreted his motor attitudes as concerned with the development of meaning, as did *W*, *A* interpreted his motor responses as having to do

with fulfilling the Aufgabe: they meant expectation or completion of the task. Had colors not dominated A's consciousness, it is most likely that his bodily reactions at the end of the experiment would have meant the fact of his recognizing the word 'good' and would have been a response to the vocal-motor image 'good.' As it was, however, the merging of the colors was the fact of his recognition-process, and the accompanying motor attitude which did not claim his attention until afterward was interpreted as a fulfilment of the task, not as a reaction which had to do with recognizing the word 'good.' Since the recognition of the word 'good' was the fulfilment of the task, recognizing and fulfilling a task are here identical. W would have chosen the first meaning of the two; A chose the second because he is synaesthetic, and the behavior of his synaesthetic imagery meant the process of recognizing. Hence the motor response meant something else. In W's case the motor attitude would have meant both, on second thought.

5. Conclusions

The real cognitive function of synaesthetic phenomena now becomes apparent: for (1) the appearance of colors in the first place constitutes developing auditory or tactual perceptions on the one hand, and (2) the further behavior of these colors constitutes shifts from one meaning to another as (for example) from letter-meanings to word-meanings; (3) the behavior of these colors acts also as a surrogate for motor responses in focal consciousness, as long as the motor phenomena are present as an unnoticed background; (4) synaesthetic phenomena behave in such fashion that they can mean the fulfilment of a task; (5) meanings fail to develop in the absence of the appropriate behavior of synaesthetic colors; (6) synaesthetic imagery constitutes the context for meaning; (7) synaesthetic images operate as a substitute for feelings of familiarity; and (8) synaesthetic images 'label' or 'interpret' the 'object,' making it meaningful.

It is significant to note that in no instance did meaning develop for any reagent until a motor attitude or visualized motor attitude attended other sensory or imaginal contents. This fact suspiciously points to the conclusion not only that kinaesthesis is an essential component of the consciousness of meaning, but also that a motor response is necessary for the development of meaning.

SERIES OF DIFFERENCE TONES OBTAINED FROM TUNABLE BARS[1]

By Paul Thomas Young

Several years ago we noticed that unusually clear and loud difference tones could be obtained by striking simultaneously two bars in the upper octave of a standard set of orchestra bells. Although difference tones may be produced throughout the entire range of the instrument, which is two and a half octaves, they are especially distinct in the region from e^4 to g^4. The top octave, c^4 to c^5, contains frequencies ranging approximately from 2000 vd. to 4000 vd.[2]

Bars make an admirable type of apparatus for group demonstrations since they are easy to manipulate, simple in construction, and further since they make it possible to produce series, musical sequences and clangs of difference tones. The resultant tones are often more easily apprehended than the generating tones themselves. Demonstrational series of difference tones, obtained from a standard set of orchestra bells, were readily heard by groups of 350 elementary students in the University of Illinois.

In the following account we shall describe (1) series of difference tones obtained from a standard set of orchestra bells, and (2) tunable bars arranged for experimental study and for the demonstration of difference tones.

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[1]From the Psychological Laboratory, University of Illinois.

[2]It is interesting to note that Ellis, translating Helmholtz, uses tones in this region for the demonstration of difference tones. He writes: "I have found that combinational tones can be made quite audible to a hundred people at once, by means of two flageolet fifes or whistles, blown as strongly as possible. I choose very close dissonant intervals because the great depth of the low tone is much more striking, being very far below anything that can be touched by the instrument itself. Thus *g* '''' being loudly sounded on one fife by an assistant, I give *f* ''''♯, when a deep tone is instantly heard which, if the interval were pure, would be *g*, and is sufficiently near to *g* to be recognized as extremely deep. As a second experiment the *g* '''' being held as before, I give first *f* ''''♯ and then *e* '''' in succession. If the intervals were pure the combinational tones would jump from *g* to *c* '', and in reality, the jump is very nearly the same and quite appreciable." Helmholtz, H. L. F., *On the Sensations of Tone*, etc., Ellis trans., 1895, 153.

I. Difference Tones Obtained from Orchestra Bells. Table I shows the approximate pitch of difference tones obtained by combining pairs of bars in the top octave, c^4 to c^5, of the instrument. The pitch of the difference tones has been estimated on the assumption that the intervals are just, while, as a fact, they are intervals of an equally tempered scale. This means that the actual pitches will in some cases be higher and in other cases lower than the values indicated in the table.

TABLE I*

	$c\#^4$	d^4	$d\#^4$	e^4	f^4	$f\#^4$	g^4	$g\#^4$	a^4	$a\#^4$	b^4	c^5
c^4	$c\#$	c^1	$g\#^1$	c^2	f^2	$g\#+^2$	c^3					
$c\#^4$		d	$c\#^1$	a^1	$c\#^2$	$f\#^2$	$a+^2$	$c\#^3$				
d^4			$d\#$	d^1	$a\#^1$	d^2	g^2	$a\#+^2$	d^3			
$d\#^4$				e	$d\#^1$	b^1	$d\#^2$	$g\#^2$	$b+^2$	$d\#^3$		
e^4					f	e^1	c^2	e^2	a^2	$c+^3$	e^3	
f^4						$f\#$	f^1	$c\#^2$	f^2	$a\#^2$	$c\#+^3$	f^3
$f\#^4$							g	$f\#^1$	d^2	$f\#^2$	b^2	$d+^3$
g^4								$g\#$	g^1	$d\#^2$	g^2	c^3
$g\#^4$									a	$g\#^1$	e^2	$g\#^2$
a^4										$a\#$	a^1	f^2
$a\#^4$											b	$a\#^1$
b^4												c^1

*Difference tones produced by minor and major sixths have been omitted from the table, since these intervals were found to generate simultaneous first and second order difference tones. This corresponds to the findings of Krueger, Meyer, and others (references at close of this paper). For purposes of preliminary demonstration it seemed best not to complicate matters through the introduction of higher order difference tones.

We might add that f^2 combined with $f\#^2$, and also $f\#^2$ combined with g^2, which are the lowest bars upon our instrument, yield unusually clear summation tones. The tone of the first combination can readily be heard as higher than that of the second.

Difference tones exaggerate the defects of equal temperament. Since the pitch of the difference tone equals the absolute vibration difference between the generators, a small change of one of the generators, say n vibrations, makes a greater relative change in the difference tone.[3]

[3] The interval c^4—$c\#^4$, for example, does not correspond to the just ratio 15/16 but to a ratio slightly short of this, 84/89. If the ratio were just, the difference tone would lie four octaves below the $c\#^4$ generato.r It is found to be flat. In general, the semitone, tone, minor third, and fifth are too short in an equally tempered scale: the major third, fourth, and tritone are too long. (Calculated, *e.g.* from data in Helmholts, *op. cit.*, Appendix, 453-456.)

We list below some of the series of difference tones which are useful for demonstrational purposes.

(1) By keeping a constant interval between the generators —as a semitone, a tone, a minor third, a major third, a fourth, a tritone, or a fifth— and by varying the generators themselves up and down the register, musical scales and simple melodies can be produced in the difference tones. If, while playing a melody, one attempts to change the interval between the generators, the disturbances which are due to temperament are at once apparent; but so long as one works with a constant interval, the difference tones have the correct tempered ratios among themselves (provided the instrument be in tune).

Chromatic series of difference tones, produced by generators which remain in a constant ratio, are to be found along the diagonals of Table I. For example, the following series of generators gives a chromatic series of difference tones:

Generator 1	$c\sharp^4$	d^4	$d\sharp^4$	e^4	f^4	$f\sharp^4$	g^4	$g\sharp^4$	a^4	$a\sharp^4$	b^4	c^5
Generator 2	c^4	$c\sharp^4$	d^4	$d\sharp^4$	e^4	f^4	$f\sharp^4$	g^4	$g\sharp^4$	a^4	$a\sharp^4$	b^4

By running down the instrument in semitones the series of difference tones becomes constantly lower and fainter and is nearly inaudible in the region of c^3. The loudest difference tones are obtained near the region from e^4 to g^4. This region, according to Helmholtz,[4] contains tones which are reënforced by the natural resonance of the ear. Generating tones above this critical region do not produce as loud difference tones as those within the region or near it. It is a fact of considerable theoretical importance that the loudest difference tones are produced from generators which receive natural resonance from the ear. We hope that further observations will be made upon this point.

(2) A descending series of difference tones may be produced from an ascending series of generators, or *vice versa*. The condition underlying such a series is that the absolute vibration difference decrease (or increase) as the generators ascend (or descend). The following series produces descending difference tones from ascending generators or, when played in the reverse order, ascending difference tones from descending generators:

Generator 1	a^4	$a\sharp^4$	b^4	c^5
Generator 2	f^4	g^4	a^4	b^4

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[4] Helmholtz, *op. cit.*, 116, 179.

(3) A constant upper generator combined with a descending lower generator gives an ascending series of difference tones; played in the reverse order, a descending series. Following is an example:

Generator 1	c^5	c^5	c^5	c^5	c^5	c^5	c^5
Generator 2	b^4	$a\sharp^4$	a^4	$g\sharp^4$	g^4	$f\sharp^4$	f^4

A constant lower generator combined with an ascending upper generator gives an ascending series of difference tones; played in the reverse order, a descending series. Following is an example:

Generator 1	$f\sharp^4$	g^4	$g\sharp^4$	a^4	$a\sharp^4$	b^4	c^5
Generator 2	f^4	f^4	f^4	f^4	f^4	f^4	f^4

Series of difference tones of approximately constant pitch (which would be of identical pitch were the intervals just) may be obtained from ascending or descending series of generators. Reference to Table I shows that the same difference tone is produced by the following combinations of generating tones:

Generator 1	f^4	a^4	c^5
Generator 2	c^4	f^4	a^4

(4) Leaps of approximately an octave (based, not upon Table I, but upon actual tests with equally tempered intervals) may be produced by the following sequences:

Generator 1	$f\sharp^4$	g^4	$g\sharp^4$	a^4	$a\sharp^4$	b^4
Generator 2	f^4	f^4	g^4	g^4	a^4	a^4

Tones approximately in the ratio 4:5:6 may be obtained from the following generators:

Generator 1	f^4	$f\sharp^4$	g^4	g^4	$g\sharp^4$	a^4	a^4	$a\sharp^4$	b^4
Generator 2	c^4	c^4	c^4	d^4	d^4	d^4	e^4	e^4	e^4

II. *Tunable Bars for Experimental Study, and for the Demonstration of Difference Tones.* In order to make an apparatus capable of delicate adjustment and accurate control, and in order to avoid the difficulties arising from the use of equally tempered intervals—difficulties which are inevitable with orchestra bells and which are magnified in the difference tones

—we have made use of the tunable bar, previously described.[5]

Round top steel bars were obtained[6] and slotted from one end to the nodal line, which is approximately .22 of the length of the bar from each end. In this slot we placed a bolt (1/16 inch diameter, ¾ inch length) which carries a lock-nut. The bolt may be moved along the slot and clamped at any point. This arrangement makes accurate tuning possible.

We found that, by striking a bar on the rounded edge, two vibratory components were produced and also their first differ-

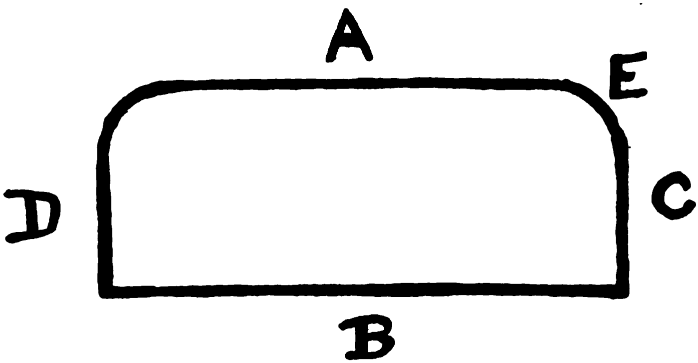

FIGURE I

ence tone. Figure I represents the cross-section of a bar in the center of its length. If the hammer strike at a point A, a tone is produced whose frequency varies directly as the thickness AB, and a blow at C produces a tone whose frequency

[5] Young, P. T., Tunable Bars, and Some Demonstrations with a Simple Bar and a Stethoscope. *Psychol. Bull.*, 1918, 15, 293 f.

Dr. C. R. Griffith has called our attention to the fact that the tone of vibrating bars is intensified by bringing a metal or cardboard funnel over the center. Although this funnel may act in part as a resonator, its chief function is probably to reduce the interference of the opposite-phase trains of sound waves which arise simultaneously from the center and ends of the bar. Intensification by means of a funnel is simpler than intensification with a stethoscope, the method which we have previously used. We might add that tunable bars have proved satisfactory for the study of beats in the elementary laboratory.

[6] The bars were purchased, at $1.00 a bar, from J. C. Deagan Co., Ravenswood and Berteau Aves., Chicago. The heavy metal hammers, necessary for striking the bars, cost 60 cents.

The bars have a cross-section of 1.1 x 2.6 cm. and, after tuning, the following lengths, in cm.:

Frequency of Bar	2600	2700	2800	3000	3200	3400	3600	3800
Length	14.1	13.5	13.4	13.0	12.6	12.3	11.9	11.6

[illegible]

[illegible] with the [illegible]

TABLE I

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]		[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]				[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]					[illegible]	[illegible]	[illegible]	[illegible]
[illegible]						[illegible]	[illegible]	[illegible]
[illegible]							[illegible]	[illegible]
[illegible]								[illegible]

Table I shows all possible difference tones [illegible] forks. The [illegible] forks [illegible] were selected [illegible] a set of forks made by Schmidt [illegible] which are tuned to the frequencies [illegible]. For [illegible] purposes it is convenient to compare fork tones and difference tones.

At the start we [illegible] to tune the forks [illegible]. We soon found, however, that when the [illegible] for example, we worked with the [illegible] two simultaneous difference tones were heard.[2] Consequently, for our purposes, it seemed best to

[1] We first tuned the [illegible] and then the [illegible] fork. Having established the frequencies of these two forks, and checked the difference tone with the [illegible] fork, we tuned the other forks [illegible] upon the basis of their difference tones. See Schmidt, [illegible] die Bestimmung hoher Schwingungszahlen durch Differenztöne, [illegible] (Wiedemann's), N. F., [illegible], LXV[illegible] [illegible] können Differenztöne [illegible] Schwingungszahlen angewandt werden?' [illegible]

[2] For discussions of the higher order difference tones see the papers by Kr[illegible], and others, references to which are appended.

limit our frequencies to those within the ratio of a fifth, 2:3 (we have actually stepped over this limit a bit). The fifth selected lies in the region where difference tones are loudest, *i.e.*, in the neighborhood of e^4 to g^4.

The following demonstrations may be readily made with the tunable bars.

(1) Difference tones of the same pitch may be produced from generators of different frequencies. If, for example, we strike any two adjacent bars, disregarding the variable and the 2700 bar, we hear a difference tone of 200 vd. Similarly difference tones of 400, 600, 800, and 1000 can be produced by several different combinations of bars (Table II). The 2700 bar makes it possible to obtain a difference tone of 100 vd. at two places, and also it gives the series: 100, 300, 500, 700, 900 and 1100.

(2) The lowest bar of the series is provided with a thumb-nut and a load which is somewhat greater than that of the other bars, so that it may be varied continuously from 2600 vd. down to 2500. If combined with the 2600 bar, it yields a continuous series of difference tones from 0 to 100 vd. If the variable bar is combined successively with the 2600, 2700 and 2800 bars, a continuous series of difference tones from 0 to 300 vd. can be produced. This type of apparatus is admirable for studying the lower limit of difference tones and for other similar problems. It would be a comparatively simple matter to construct a series of tunable bars which would yield a continuous series of difference tones from 0 to 1200 vd. or above.

TABLE III

Ratios	Difference Tones	Generators			
1	50	2550	2600		
2	100	2600	2700		
3	150	2550	2700		
4	200	2600	2800		
5	250	2550	2800		
6	300	2700	3000		
1	100	2700	2800		
2	200	2800	3000		
3	300	2700	3000		
4	400	2800	3200		
5	500	2700	3200		
6	600	2800	3400		
1	200	2600	2800	2600	3800
2	400	2600	3000	2800	3800
3	600	2600	3200	3000	3800
4	800	2600	3400	3200	3800
5	1000	2600	3600	3400	3800
6	1200	2600	3800	3600	3800

(3) Series of difference tones, having the simple ratios 1:2:3:4:5:6 may be readily produced. Table III shows three such series, the first being obtained by setting the variable bar at 2550 vd. If we call a tone of 50 vd. *C*, then the following sequence may be produced by combining the tones shown in Table III:

C c g c′ e′ g′ c′′ e′′ g′′ c′′′ e′′′ g′′′

Here is a musical sequence extending through four and a half octaves produced by nine bars whose frequencies are all included within the interval of a minor sixth.

(4) Musical clangs, made up of difference tones, may be produced by striking simultaneously or in rapid succession bars which give only the small ratio numbers (1, 2, 3, 4, 5, 6). Table

TABLE IV

Clang No. 1 Bars 2600 2800 3000 3200 3400 3600 3800
Difference tones: 200, 6 times; 400, 5 times; 600, 4 times; 800, 3 times; 1000, 2 times; 1200, 1 time.

Clang No. 2 Bars 2600 2700 2800 3000 3200
Difference tones: 100, 2 times; 200, 3 times; 300, 1 time; 400, 2 times; 500, 1 time; 600, 1 time.

Clang No. 3 Bars 2550 2600 2700 2800
Difference tones: 50, 1 time; 100, 2 times; 150, 1 time; 200, 1 time; 250 1 time.

IV shows three such clangs. If one play successively these three clangs in the order 1, 2, 3, a series of musical chords will be heard, each one deeper in pitch and richer than the preceding. It should be remembered that to produce a musical clang of difference tones the generators themselves must stand in such simple numerical ratios as 13:14:15:16:17:18:19 or 26:27:28:30 or 51:52:54:56. We have calculated that with comparatively few bars it is possible to get a complete scale of difference tones in just temperament, and also from the same bars to produce difference tone clangs related as tonic to dominant, or as tonic to subdominant.

If the bars are struck energetically, the clang has a piercing quality which may be almost painful, and which is localized in the ear. When the bars are played lightly, or with a moderate blow of the hammer, the musical chord is rich, full and true while the total clang is very bright, on account of the loudness and high pitch of the generators. Difference tone clangs are agreeable and, we believe, are not without musical value.

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N.B. The C. H. Stoelting Co., 3037-3047 Carroll Ave., Chicago, are manufacturing sets of bars of the sort described in the present paper for the demonstration and experimental study of difference tones. Also they are preparing differential bars for work with beats and the tonal *DL* (see foot-note 5). At the present time no price can be mentioned.

THE HYDROGEN ION CONCENTRATION OF THE MIXED SALIVA CONSIDERED AS AN INDEX OF FATIGUE AND OF EMOTIONAL EXCITATION, AND APPLIED TO A STUDY OF THE METABOLIC ETIOLOGY OF STAMMERING[1]

By Henry E. Starr

Psychology today is finding in reflex arcs and reaction patterns definite foundation-stones such as chemistry obtained in the concepts of atom and molecule. Behind the simple reflex, however, as well as behind the more complex performance, we find as prime determinants the amount of energy at the disposal of the individual and his degree of excitability. Energy and excitability, mutual interdependents to a considerable extent, condition both quality and quantity of performance, even when the latter appears to the unanalytical to be determined solely by motivation. And what is "motivation", that mysterious "mover" which the dualists call "soul", manifesting itself in hope and fear, desire and aversion, love and hate,—a labyrinthine complexus of interacting instincts, emotions, apperceptive residua, and bodily tone? May it not be considered as the resultant harmony or discord of the play, energetic or feeble, anabolic or catabolic, of metabolism upon anatomical structure?—harmony or "normality", if it blends with the environment; discord or "abnormality," if the pitch be too high or too low?

I have spoken of motivation as the resultant of the play of physiological processes. I do not insist upon that as a final mechanistic definition. For the purposes of this study I am quite willing to accept as a working hypothesis that the physiological processes are the result of psychical stimuli. That a misbehaving liver may spoil one's temper, or that a chronic ill-temper may affect the liver, may either or both be tritely true. What at this time I wish to call especial attention to is the fact that even motivation, that most psychical of psychological processes, is correlated with or parallels metabolism. Correlation of behavior with bodily tone is obvious in any performance. Efficiency, intellect, intelligence, all are being measured today by means of standardized tests, as a result of which the psychological examiner grades the performer on a relative scale, more or less accurately,—the degree of accuracy depending primarily upon the psychological diagnostic ability of the examiner. It is patent even here, however, that bodily tone, energy at the disposal of the performer, excitability, and general motivational attitude of the subject determine to some extent the amount of product he will turn out in a unit of time, his ability to put to use knowledge already acquired, his ability to solve what

[1]From the Psychological Laboratory and Clinic and the Robert Hare Chemical Laboratory of the Medical School, University of Pennsylvania.

is for him a new problem.[2] In every instance the physiological factor is evident. We may cling to the subterfuge of psychophysical parallelism, admit no interaction between the mental and the physical, regard them as such absolutely distinct categories that nothing of either can stand in a causal relation to anything in the other. But we must admit that the parallelism is frequently so close that between the lines there is scarce the width of Omar's false from true dividing hair.

Avoiding anything of a controversial nature, however, it is sufficient to note that the physiological may serve as an index of the psychological. This was pointed out by Bell in his "Anatomy and Philosophy of Expression," and the thesis was subsequently developed more scientifically by Darwin in his study of "Expression of the Emotions in Man and Animals." It is, in fact, the initial premise of the psychological examiner who observes a performance and interprets it in terms of thought, intellect, intelligence, etc. And if psychologists today are able thus to diagnose the mental conditions by observing gross bodily movements, what *finesse* they would gain by the employment of tests indicating quickly and accurately the metabolic status, degree of emotional stress, and energy at the disposal of the individual, irrespective of any attempts at malingering or excessive performance on the part of the subject, manifesting themselves in overt acts difficult of interpretation by a purely psychological procedure. Such indices, obviously, will have to be developed along the lines of the science which is employed for purposes of metabolic diagnosis, *i. e.*, of physiological chemistry. Physiology is the connecting link between psychology and chemistry. We need not go so far as to assert that psychology is a "refined physiology." We need simply to recognize the fact that the physiological parallels the psychological and is indicated by the chemistry of the individual.

In this connection Pawlow[3] merits special mention as having first reported the effect of purely psychical stimuli upon the digestive glands resulting in chemical differentiation of secretion. Cannon's work on the adrenals is a chemical study of the great emotives: pain, hunger, fear, and rage.[4] Crile's interesting and important attempt to portray man as an "adaptive mechanism"[5] would give to human behavior the foundation of a chemically activated kinetic system, consisting of brain, thyroid, adrenals, liver and muscles. Sajous,[6] Laignel-Lavastine,[7] and other investigators of the internal secretions have proved that the metabolism of the ductless glands is the chief etiological factor in many "mental" conditions.

Meanwhile, particularly along the lines of colorimetry and nephelometry, physiological chemistry has been rapidly developing an ever more delicate and accurate technique for the detection and determination of the products of metabolism. This is especially notable in the field of hydrogen ion determination, thanks largely to the work of Sørenson Palmer and Henderson, Clark and Lubs, and Cullen and Van Slyke. The time

[2]Definitions respectively of efficiency, intellect and intelligence, adapted from Witmer, Reference Book in Clinical Psychology and for Diagnostic Teaching, *Psychological Clinic*, May 15, 1919.

[3]Pawlow, J. P., The Work of the Digestive Glands. Trans. by W. H. Thompson. London, 1902.

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[7]Laignel-Lavastine, M., The Internal Secretions and the Nervous System. New York and Washington, 1919.

is ripe for the application of the exact methods of chemical science to the problems of metabolism conditioning (or indicating) mental states. A psychological chemistry is due, from which may ultimately be developed a chemical psychology.

The material to be employed for chemical analysis, correlated with mental diagnosis, obtainable from a living human, may be the blood, urine, faeces, gastric juice, or saliva. As a means of investigating the metabolism of an individual the blood appears most admirable, in that it is itself virtually living tissue of the organism and carries the secretions, hormones, end-products of digestion, etc. Unfortunately, the popular fear of bleeding would no doubt militate against the usefulness of blood-tests as routine clinical methods of psychological diagnosis, and the fear reaction *per se* might so alter the blood as to render it of no diagnostic value with regard to the motivational or emotional *status quo*. The urine is accessible more readily and in greater quantity than is the blood. But it represents the waste-products taken from the blood by the kidneys and, in many cases, altered in transit. As such it is not a direct index of immediate metabolic condition such as manifests itself in more or less transient mental or psychical states. (Of course the general metabolic condition of the body may be frequently inferred from these waste-products.) The faeces, apart from the fact that they obviously would never become widely-employed clinical material, are contaminated by undigested food, and would furnish no index of material assimilated and thus affecting the economy, without an accurate preliminary analysis of the food ingested. Nor would they be of value as indices of transient emotional states, etc. To the gastric juice and stomach-contents objections may be raised similar to those advanced with respect to the faeces.

There remains the saliva. This fluid is constantly being secreted, swallowed, and passed through the physiological cycle. It may be readily collected for examination at all times and places. It may be regarded practically as transformed protoplasm of the secreting cell, with admixture of salts and other substances virtually dialyzed from the blood, and affected to a greater or lesser degree by the conditions obtaining in the oral cavity and by the constituents of the alveolar air. The glands of secretion have abundant neural connections with both the cranial and the sympathetic nervous systems. Thus of the three principal sets of salivary glands—sublingual, submaxillary and parotid—each one is innervated by both cranial and sympathetic nerves. The chorda tympani connects with the submaxillary and sublingual glands. The auriculo-temporal branch of the trifacial nerve supplies the parotid. As to the sympathetic fibres, to quote Fischer,[8] they "are derived in the

[8] Fischer, M. H., The Physiology of Alimentation. New York, 1907.

main from the second, third and fourth thoracic nerves, which, after passing into the sympathetic chain, ascend to the superior cervical ganglion, from which nerve fibres............are given off that, after following the external carotid artery, are finally distributed to the various salivary glands." The amount and composition of the secretion of a given gland vary according to the nerve stimulated, as noted particularly by Heidenhain and by Langley. In his everyday experience the clinical psychologist observes the drooling of the idiot, and frequently the dry mouth of fear, the latter being especially exemplified by the well-known Chinese "ordeal of rice." Thus the saliva suggests itself as a possible index of metabolic conditions manifesting themselves in mental or emotional states.

That the salivary composition and reaction vary in health and disease has been reported by various investigators. Almost a century ago (1835) Donne noted that the saliva was acid to litmus in certain diseases, including encephalitis, whereas he regarded it as normally alkaline.[9] Today Kirk is the foremost protagonist of the diagnostic value of the saliva, especially with reference to metabolic factors in the etiology of dental caries,[10] and his kindly advice has been a distinct aid in the present research.

When, early in 1920, the Directors of the Psychological Laboratory and Clinic of this university asked the writer as to the feasibility of employing the salivary reaction as an adjunct in clinical psychological diagnosis, his first step was a survey of the biochemical literature bearing upon the problem.[11] He found that while many, and contradictory, findings had been reported as to the alkalinity and acidity of the mixed saliva under various conditions, much of the work had been of the crude litmus paper type. The more carefully conducted examinations had been made principally by titrational quantitative methods, in which field the work of Gies has been most intensive.[12] In titrational quantitative determinations the degree of acidity or alkalinity reported expresses conversely the quantity of alkali or acid required to be added to a definite quantity of the "unknown" (in this instance the saliva) in order to render the resultant solution neutral. The saliva, however, is an amphoteric liquid, *i. e.*, within certain limits it will act as an alkali to neutralize acids or as an acid to neutralize alkalis. Consequently titrational methods give no direct determination of the *status quo* of the intensity factor of the "acidity," which arises from the quantity of ionic hydrogen present in the solution. More recent chemical research in various biological fields, however, has resulted in the discovery that in the majority of instances the hydrogen ion concentration is a greater determinant of certain life phenomena than is titratable acidity. That is, the quantity of ionic hydrogen present in a given volume of the solution under examination is frequently a more important biological factor than is the quantity of hydrogen which may be replaced in the course of titration with an alkali.

It may not be out of place to note here that a hydrogen ion is an atom of hydrogen bearing a single positive electric charge; *i. e.*, it is a univalent anion. A hydroxyl ion (OH^-) is a chemical radical bearing a single negative

[9]Donne, A., *Arch. génér. de Med.*, May 1835.

[10]Kirk, E. C., *The Dental Review*, May 1903.

[11]For a review of the biochemical literature, see Starr, H. E., Biochemical Studies of Human Mixed Saliva. I.

[12]*Op cit.*

electric charge; *i. e.*, it is a univalent cation. When a substance is dissolved, it dissociates to a greater or lesser extent into anions and cations. Thus in the case of water (H_2O) itself there is a certain amount of dissociation, as expressed by the equation: HOH $\leftrightarrows$ $H+ + OH^-$. The lines of the equality sign are arrow-tipped in opposite directions to indicate that the reaction is reversible. The ratio of the product of the concentrations of the anions and cations to the concentrations of the undissociated molecular portion is a constant for a given salt in a given solvent at a given temperature. Thus, if

(A) = concentration of anion,
(C) = concentration of cation,
(AC) = concentration of undissociated molecules,
K = a constant,

then $\frac{(A) \times (C)}{(AC)} = K.$

In pure water, hydrogen furnishes the anion (H+) and hydroxyl the cation (OH^-), while HOH is the formula of the undissociated molecule, hence

$$\frac{(H+) \times (OH^-)}{(HOH)} = K.$$

Pure water, however, is so slightly dissociated [*i. e.*, (HOH) is so extremely great in proportion to $(H+) \times (OH^-)$] that for all practical purposes we may discard the denominator, substitute another constant (k^w), and write our equation as:

$(H+) \times (OH^-) = k_w$. Here (H+) represents the quantity of ionic hydrogen and (OH^-) the quantity of hydroxyl ions. By electrometric determinations it has been found that for perfectly pure neutral water at 21° C. the value of k_w is 10^{-14}; and as in a neutral solution there must be an equal quantity of hydrogen ions and of hydroxyl ions, it follows that 1 liter of pure neutral water contains 10^{-7} grams of each kind of ions. Consequently if, at 21° C., a solution contains less than 10^{-7} gms. of H ions per liter, it contains more than 10^{-7} gms. of OH ions per liter, and is therefore "alkaline." Conversely, if it contains more than 10^{-7} gms. of H ions per liter, it must contain less than 10^{-7} gms. of OH ions per liter, and is therefore "acid." Or, perhaps more simply, if a solution contains more H+ ions than OH^- ions, it is called "acid"; if it contains more OH ions than H ions, it is called "alkaline." But whether acid or alkaline it must contain some H ions, and consequently we may scale all solutions in terms of greater or lesser hydrogen ion concentration. As Sørenson pointed out, log $_{10}\frac{1}{(H)}$ may be used to express a given hydrogen ion (H ion, or hydrion) concentration in lieu of (H+). He suggested for this the symbol P_H^+. This Sørenson negative logarithmic notation, as it has been termed, is now quite widely adopted, the symbol having been simplified, however to pH. Thus a H ion concentration of 10^{-7} is written simply as pH 7.00. Or, for example, a solution of acetic acid, containing 6.0 grams of acetic acid per liter, has a H ion concentration of 1.36×10^{-3} which equals $10^{0.133-3}$ or $10^{-2.867}$, and which may be expressed as pH 2.867. Thus pH = $-\log_{10}$ (H+). Because of the greater simplicity of the Sørenson notation the writer has employed it in the present research. It must be borne in mind throughout that the hydrogen ion concentration varies inversely as to direction with the pH; *i. e.*, a low pH indicates a high H ion concentration, and a high pH indicates a low H ion concentration. Consequently when the "salivary pH" is spoken of as increasing, the hydrogen ion concentration of the mixed saliva is decreasing, and conversely.

Prior to engaging in the present research, the writer found it necessary to make a series of "Biochemical Studies of Human Mixed Saliva" in the course of which was developed a technique for the colorimetric determination of the hydrogen ion concentration of the mixed saliva, or "salivary pH." The general procedure is to collect the saliva without the aid of a stimulus (as the chewing of inert substances was found to raise the salivary pH),[12] to make the determination immediately after ejection of the specimen without allowing it to stand or centrifuging it (as each of these measures was found to make for decreased salivary pH),[13] and to employ 1 cubic cm. of the saliva for a determination, which is made by adding dibromothymolsulphonephthalein or phenolsulphonephthalein to the saliva diluted with water of an H ion concentration of pH 6.6—6.7, and comparing the resultant virage with those obtained by similar treatment of standard solutions of known pH.[14]

It is obvious that the actual quantity of ionic hydrogen detected by the colorimetric determination of hydrogen ion concentration is quite small. Thus, there is required but 1 cubic cm. of saliva in the technique employed throughout this investigation. Consequently, if the hydrion concentration of the saliva is reported as 6.00, which means that one liter of the saliva contains 1/1,000,000 gram of ionic hydrogen, there has been actually determined in the one cubic cm. employed 1/1,000,000,000 gram of ionic hydrogen. Similarly, when the hydrogen ion concentration is reported as pH 7.00, it means that there has been determined 1/10,000,000,000 gram of ionic hydrogen in the 1 cubic cm. It may be of interest to note that a solution of pH 6.00 is indicated by a pale yellow color when phenolsulphonephthalein is used, and by a "grass green" when dibromothymolsulphonephthalein is employed; whereas a solution of pH 7.00 displays a distinctly red color with the former and a decided blue with the latter indicator. The color changes in the lower range are better indicated by the latter than by the former; for the higher ranges, the reverse is true.

Having thus an adequate technique, the next step was the determination of the hydrogen ion concentration of 610 specimens of human mixed saliva collected from 228 healthy normal subjects. The 610 specimens included 5 specimens each from 41 individuals; 4 specimens each from 6 individuals; 3 specimens each from 19 individuals; and 2 specimens each from 162 individuals. The range was found to be from pH 5.95 to pH 7.25, with 86.6% of the specimens within the limits of pH 6.55 to pH 7.00 inclusive. The mean was pH 6.78; the median, pH 6.80; and the mode pH 6.80 to pH 6.90 inclusive. The results of this investigation, in terms of relative frequency, are presented in Table 1, and also in Graphs 1 and 2. It was noted that in general, when the salivary pH was 6.60 or less, the individual appeared fatigued or deficient in the amount of energy at his disposal; when the salivary pH was about 7.00 the individual appeared to have an abundance of energy at his disposal.[15]

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[13] *Op. cit.*

[14] *Op. cit.*

[15] *Op. cit.*

Table 1. The Hydrogen Ion Concentration of the Mixed Saliva of Normal Healthy Individuals, in Terms of Relative Frequency.[16]

Hydrogen ion concentration of the mixed saliva	Relative frequency
pH	%
5.95	0.2
6.00	0.5
6.05	——
6.10	0.3
6.15	0.2
6.20	0.3
6.25	0.3
6.30	0.2
6.35	0.5
6.40	1.0
6.45	1.6
6.50	3.0
6.55	4.9
6.60	6.7
6.65	6.4
6.70	9.2
6.75	9.8
6.80	11.6
6.85	12.1
6.90	11.5
6.95	7.9
7.00	6.4
7.05	1.5
7.10	1.3
7.15	1.3
7.20	0.8
7.25	0.5

Following the determination of the normal physiological range of salivary hydrion concentration, experiments were made to ascertain whether or not a diurnal rhythm might be found.[17] Twenty-two salivary pH determinations were made at intervals during a day of inactivity and total abstinence from food, 22

[16]Adapted from Starr, Biochemical Studies of Human Mixed Saliva. The values of salivary pH reported are probably somewhat higher than the actual values obtaining in the oral cavity, inasmuch as in the method employed it was impossible to avoid a slight loss of CO_2 during the determinations with concomitant increase in pH of from 0.05 to 0.15 pH. This, of course, does not affect comparisons made between the values of pH found or the conclusions drawn therefrom.

[17]*Op. cit.*, II.

on a day of inactivity when a mixed diet was ingested, 16 on a day of normal activity and total abstinence from food, 14 on a day of normal activity when a mixed diet was ingested. A tendency toward a rhythm was noted; but the most marked and constant finding was a steady decrease in salivary pH from about 2.00 P. M. until the ingestion of dinner at 6.00 P. M. This result tended to confirm a tentative conclusion drawn during the preliminary investigation of 228 subjects, *viz.*, that the hydrion concentration of the mixed saliva increased when the individual was fatigued or lacked energy. Accordingly a series of 88 determinations[17] of salivary pH were made on 7 individuals during days of normal activity and mixed diet. The findings pointed practically invariably to a steady decrease in salivary pH during the afternoon, *i. e.*, toward the close of the working day when the individual's energy was ebbing and the products of fatigue, the principal of which is carbon dioxide, were accumulating in his tissues and blood.

In the course of the latter investigation, however, it was noted on several occasions that when a subject was emotionally excited his salivary pH increased regardless of the time of day. Thus subject I.G. at 3.00 P. M. had a salivary pH of 6.70; following some fairly strenuous exertion in the pursuit of his work, his salivary pH dropped by 4.00 P. M. to 6.60. At 5.00 P. M., just after the extinguishment of a slight fire at which he had been present, his salivary pH had increased to 7.20. By 5.30 P. M. it had subsided to 6.90, by 6.00 P. M. to 6.70, and by 6.30 P. M. to 6.60. Similarly M. S., having shown a salivary pH of 6.95 at 2.30 P. M., and a decrease to 6.80 at 3.30 P. M., having been angered about 4.15 P. M. by an individual against whom he dared not display his anger, showed at 4.30 P. M. a salivary pH of 7.25, which, as he grew calmer, subsided by 5.00 P. M. to 6.85 and at 5.30 P. M. had dropped to 6.65. Subsequent to these determinations subject I.F.T. one morning became quite angry. His salivary pH had just been found to be 6.75 at 9.00 A. M. At 9.15, when storming about quite angrily, his salivary pH was again determined, and found to have increased to 6.95. After he realized that he had misunderstood certain matters and that there was no real cause for anger, his salivary pH dropped by 9.32 A. M. to 6.80.

As a "check" on the preceding findings, the following experiment was performed. At 10.08 A. M. a 2 cubic cm. specimen of mixed saliva was collected from G.X.T., an adult male subject, known by the experimenter to be somewhat excitable. The pH of the specimen was 6.90. At 10.13 A. M. the subject was given misinformation calculated to anger him. It resulted as anticipated. At 10.28 A. M., at the height of his rage, he was commanded to "spit" which he did unthinkingly and with ve-

hemence. The pH of this specimen was 7.40. Unfortunately, the explanation of the operator, that the whole affair was staged as an experiment, did not result in any immediate calmness on the part of the subject. He left the laboratory in a rage, but returned about 1.00 P. M., when his salivary pH was found to be only 6.75. The results of the experiments upon normal individuals when laboring under emotional excitement [18] will be found tabulated in Table 2.

Table 2. The Increase in Hydrogen Ion Concentration of the Mixed Saliva Concomitant with Emotional Stress, in Normal and Healthy Subjects

Subject	Prior to Excitement Hydrogen ion concentration of the mixed saliva		During Excitement Hydrogen ion concentration of the mixed saliva		After Excitement had subsided Hydrogen ion concentration of the mixed saliva	
	Time	pH	Time	pH	Time	pH
IG	4.00 P. M.	6.60	5.00 P. M.	7.20	6.00 P. M.	6.70
MS	3.30 P. M.	6.80	4.30 P. M.	7.25	5.30 P. M.	6.65
IFT	9.00 A. M.	6.75	9.15 A. M.	6.95	9.32 A. M.	6.80
GXT	10.08 A. M.	6.90	10.28 A. M.	7.40	1.00 P. M.	6.75

Table 2 shows that under emotional stress four healthy normal individuals, whose average initial salivary pH was 6.75, displayed a decrease in hydrion concentration of the mixed saliva resulting in an average salivary pH of 7.20 which dropped, after they became calm, to an average of 6.73. This may not at first sight appear to be a very great change numerically; but it must be remembered that pH is the negative log to the base 10 of the actual hydrogen ion concentration. The *relative* increase is thus seen to be very great.

The writer next turned his attention to the chemical cause of the hydrogen ion concentration of the mixed saliva, with the result that he has found it to be due primarily to carbon dioxide.[19] This finding at once suggested the carbon dioxide content of the alveolar air, and therefore of the venous blood, as of etiological import. Accordingly 34 determinations of salivary pH in the usual manner, and simultaneous determinations of the carbon dioxide content of the alveolar by means of the Fridericia method,[20] were made on seven individuals, with the result that a definite correlation was found to exist between the salivary pH and the carbon dioxide content of the alveolar air,—the greater the former, the less the latter; *i. e.*, in every

[18]Compare with *op. cit.*

[19]In more correct chemical terminology it is due to the ratio $\frac{H_2CO_3}{BHCO_3}$ in which B represents a univalent base, according to the terminology of Van Slyke, and is to be so understood throughout. Cf. *op. cit.*, I.

[20]For detailed explanation of the Fridericia method for the determination of the carbon dioxide content and tension of the alveolar air, see Hawk, P. B., Practical Physiological Chemistry. 7th ed., Phila.

instance the H ion concentration of the mixed saliva was found to vary directly with the carbon dioxide content of the alveolar air.[21]

The correlation between the carbon dioxide content of the alveolar air and that of the saliva, the latter manifesting itself in hydrion concentration, suggested that a thorough ventilation of the lungs might result in decreased hydrion concentration of the saliva, especially inasmuch as Van Slyke[22] has found that the carbon dioxide of the blood may be "blown off" by voluntary deep breathing. Accordingly, the salivary pH of ten subjects was determined; and they were then placed out-of-doors and required to breathe "deeply and vigorously" for 10 min., when their salivary pH was again determined in the usual manner. It was found that in every instance the pH had, at least temporarily, decreased, indicating a decrease in the hydrion concentration of the mixed saliva.

The intimate association found in the course of these investigations between the hydrogen ion concentration and carbon dioxide content of the mixed saliva and the carbon dioxide of the alveolar air, in relation to concomitant states of fatigue and emotional excitement, leads to innumerable problems of relationship between breathing habits, mental and physical states, and all the ramifying sequelae of the hydrogen ion concentration of bodily fluids, respiration, carbon dioxide content of the blood, the functioning of the adrenals, and so on well nigh *ad infinitum.*

These biochemical and psychochemical findings were discussed with Dr. E. B. Twitmyer, Professor of Psychology and Director of the Clinic for Speech Defects of this University. Dr. Twitmyer has noted for many years that a large number of stammerers have very little chest expansion and may be denominated as "sub-breathers." Some—comparatively few—do not show this defect in the regular clinical examination, but are distinctly psychopathic, others are both psychopathic and sub-breathers. Very few indeed do not fit into one or the other of these categories. Still fewer are not remedied by means of corrective measures which he employs, including a definite series of breathing exercises, carefully calculated to increase the stammerer's use of his lungs.

In this connection it will be recalled that Halle,[23] Gutzmann,[24] Ten Cate,[25] Fletcher[26] and others have studied the breathing habits of stam-

[21] *Op. cit.*

[22] Van Slyke, *Jnl. Biol. Chem.*, 1921, 48, 153.

[23] Halle, *Monats. f. Sprachheilkunde*, X, 1900, 225.

[24] Gutzmann, H., *Monats. f. Sprachheilkunde*, 1908, XVIII, 179. Has also written more than 20 other articles along similar lines.

[25] Ten Cate, M. J., *Monats. f. Sprachheilkunde*, 1902, XII, 247 and 321.

[26] Fletcher, J. M., this JOURNAL, 1914, XXV, 201 ff.

merers in various ways, primarily with reference to the effect upon speech of the gross bodily movements of breathing. One chief difficulty has been that each has apparently attempted to make all stammerers fit into some one specific type, and has not recognized the importance of individual differences and the existence of more than one factor in the etiology of the the defect. Investigators of stammering other than those already cited have apparently neglected the physiological factors entirely, and have applied psychological clinical tests and measurements to stammerers, generally with negative findings. Such an approach is, of course, of a totally inadequate nature. It may be of interest to know the degree of visual imagery or of the intelligence of a stammerer, but—if an individual habitually stammers, he is a stammerer and the stammering *per se* is all-sufficient to diagnose him as a stammerer. Following the diagnosis, in a well regulated clinic, comes the prognosis and prescription of treatment. And here is raised the problem of the metabolic etiology of stammering. It is the etiological factors capable of control which are of primary importance. Dr. Twitmyer has found that certain breathing exercises, together with other drill work, result almost invariably in improvement of the general bodily tone and concomitantly of the speech of stammerers for whom he has prescribed them, on the basis of his diagnosis of them as sub-breathers. Accordingly, at his suggestion and with his cooperation, the writer applied the technique and findings already summarized to a specific study of stammerers applying to his clinic for treatment.

The primary purposes of the research were:

1. (*a*) To determine whether or not such stammerers as showed evidence, in physiological and psychological examination, of being sub-breathers were actually overloaded with carbon dioxide, which would be indicated by the hydrogen ion concentration and carbon dioxide content the mixed saliva; and conversely

(*b*) To determine if the hydrogen ion concentration of the mixed saliva, in connection with determinations of its carbon dioxide content, might be employed as a diagnostic and prognostic aid in the recognition of sub-breathers as a type.

2. (*a*) To determine whether or not such stammerers as were distinctly psychopathic, and in consequence probably more or less constantly in a state of emotional stress, would show a characteristically low hydrogen ion concentration of the mixed saliva; and conversely

(*b*) To find out if the hydrogen ion concentration of the mixed saliva might be employed as an index of more or less chronic emotional disturbance.

3. (*a*) To determine whether or not hyper-sensitive or hyper-excitable subjects would give evidence of their excitement upon the application of a normally inadequate stimulus, by decrease in the hydrion concentration of the mixed saliva; and conversely

(*b*) To determine if decrease in salivary hydrion concentration might be employed in general as an index of transient emotional excitement.

In more general terms:

1. To investigate the metabolic etiology of stammering, and

2. To ascertain the degree of usefulness of determinations of the hydrogen ion concentration and concomitant carbon dioxide content of human mixed saliva in psychological examinations.

With these specific purposes in view the following research was conducted.[27]

1. *Are clinically diagnosed sub-breathing stammerers overloaded with carbon dioxide, and may the hydrogen ion concentration, in connection with the carbon dioxide content of the mixed saliva, be employed as an index of sub-breathing?* Fifty-eight subjects applying to the Speech Clinic for treatment and diagnosed as sub-breathers were examined as follows. The subject was seated in a comfortable chair and told to allow a mouthful of saliva to collect in his mouth, without chewing or other stimulation of the salivary glands. At the expiration of 5 min. he was directed to eject the accumulated saliva into a 15 ml. graduated centrifuge tube. The hydrogen ion concentration of the saliva was at once determined as in the preliminary survey of normal individuals previously described, dibromothymolsulphonephthalein being employed as indicator.[28] Subsequent to the determination of the hydrion concentration, the diluted saliva to which the indicator had been added was aerated in an apparatus previously employed in the biochemical investigations,[29] which involved passing the air through a series of Woulfe bottles and Liebig bulbs containing respectively concentrated sulphuric acid, 30 % sodium hydroxide solution, a tube containing fragments of solid sodium hydroxide, and a small flask containing distilled carbon dioxide free water, prior to it (the air) reaching the saliva through which it bubbled, and washing out the free and loosely combined carbon dioxide which was conveyed by suction into N/5 barium hydroxide solution. The aeration was continued for 45 min., resulting in the formation and separation of a white precipitate of barium carbonate in the tube containing the barium hydroxide solution. Great care was taken throughout to prevent any possible admission of air containing carbon dioxide into the apparatus, and consequently the exact quantity of carbon dioxide thus washed out of the saliva and trapped by the barium hydroxide solution could be ascertained by careful titration of the latter before and after aeration.[30] In every instance, before the period of aeration was completed, the virage of the saliva (with indicator) reached

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[27]All of the determinations as regards both sub-breathing and psychopathic stammerers were made upon the subjects when they applied to the Speech Clinic for treatment and *before* they had received corrective treatment. The present research is distinctly with reference to the metabolic etiology of stammering, and the diagnostic value of the salivary pH index. A subsequent research will deal with the results of the treatment given the stammerers by Dr. Twitmyer, as indicated by change in hydrion concentration and carbon dioxide content of the mixed saliva.

[28]Starr, H. E., Biochemical Studies of Human Mixed Saliva, I.

[29]*Ibid.*

[30]*Op. cit.*, I.

or present neutrality (pH 7.00), while the barium hydroxide solution became turbid, indicating the removal of the carbon dioxide from the saliva. The quantities of carbon dioxide trapped by the barium hydroxide and precipitated as barium carbonate need not be reported, inasmuch as in every instance, as just noted above, the hydrogen ion concentration was found to be due to the ratio of free to combined carbon dioxide.[22]

Upon the saliva of the 58 subjects mentioned, 200 determinations were made, distributed as follows: 1 determination each on 28 subjects; 2 determinations each on 3 subjects; 5 determinations each on 20 subjects; 7 determinations each on 3 subjects; thus totalling 200 determinations on 58 sub-breathing stammerers. The results are shown in terms of relative frequency in Table 3.

It is evident from the results presented in Table 3, and shown very clearly in Chart I, that the sub-breathing stammerers in general and in particular are overloaded with carbon dioxide in their saliva far in excess of the normal individual. Thus the mean, mode and median for the salivary pH of the sub-breathers are respectively 6.00, 5.95 to 6.10, and 6.05, whereas for normal individuals they are respectively 6.8, 6.8 to 6.9 and 6.8. Obviously, therefore, the salivary pH in conjunction with determination of carbon dioxide content by aeration may be employed as an aid in the diagnosis of sub-breathing subjects. That in this series of determinations the salivary pH alone could have been so employed without regard to determination of carbon dioxide content, inasmuch as the latter was always the chief determinant of the former, must by no means be construed as indicating that the latter determination may be safely dispensed with. In the examination of certain distinctly pathologic salivas the writer has found lactic acid present which led to a fairly high hydrogen ion concentration and consequent low pH of the saliva even after prolonged and vigorous aeration, in the course of which comparatively little carbon dioxide was given off. This contingency may be seldom met with, but its possibility renders it absolutely necessary that the saliva be subjected to aeration or other adequate method of determination of carbon dioxide content in connection with the determination of the hydrogen ion concentration of the saliva, if the latter is to be of any diagnostic value whatever.

2. Do psychopathic stammerers show a characteristically low hydrogen ion concentration and carbon dioxide content of the mixed saliva, and may these salivary biochemical characteristics be employed as indices of chronic emotional excitement?

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22 *Note 19 supra.*

Table 3. The Hydrogen Ion Concentration of the Mixed Saliva of Sub-breathing Stammerers, in terms of Relative Frequency

Hydrogen Ion concentration of the mixed saliva pH	Relative frequency %
5.4 minus[33]	2.0
5.40	1.0
5.45	1.5
5.50	1.5
5.55	2.0
5.60	1.5
5.65	0.5
5.70	2.0
5.75	4.0
5.80	4.5
5.85	7.0
5.90	7.5
5.95	9.0
6.00	10.0
6.05	8.5
6.10	7.5
6.15	7.0
6.20	6.5
6.25	5.0
6.30	3.5
6.35	2.5
6.40	2.0
6.45	0.5
6.50	1.0
6.55	0.5
6.60	1.5

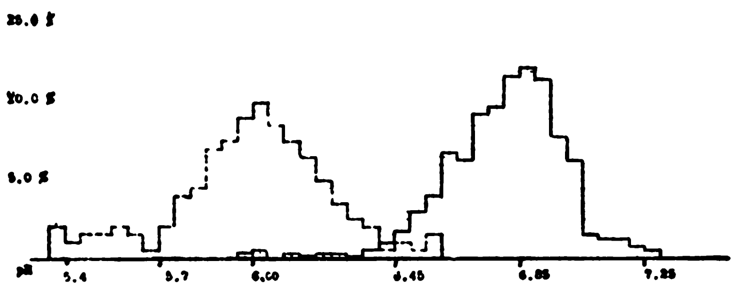

CHART 1. The hydrogen ion concentration of the mixed saliva of sub-breathing stammerers and of normal individuals
Abscissa: Hydrogen ion concentration in terms of pH
Ordinate: Relative frequency in terms of per cent
———————— indicates normal individuals
— — — — — — indicates sub-breathing stammerers

[33] Inasmuch as the lower limit of accuracy of the indicator employed is pH 5.40, specimens showing a virage indicating a lower pH are reported imply as "pH 5.40 minus."

Table 4. The Hydrogen Ion Concentration of the Mixed Saliva of Psychopathic Stammerers, in Terms of Relative Frequency

Hydrogen ion concentration of the mixed saliva	Relative frequency
pH	%
6.90	2.0
6.95	2.0
7.00	---
7.05	---
7.10	---
7.15	2.0
7.20	4.0
7.25	---
7.30	4.0
7.35	2.0
7.40	4.0
7.45	6.0
7.50	6.0
7.55	10.0
7.60	14.0
7.65	12.0
7.70	10.0
7.75	8.0
7.80	6.0
7.85	2.0
7.90	4.0
7.95	---
8.00	2.0

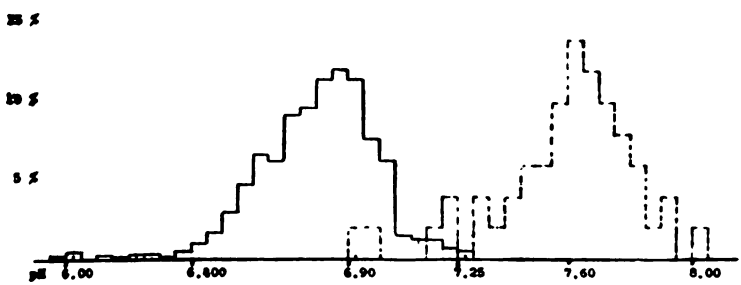

CHART 2. The Hydrogen Ion Concentration of the Mixed Saliva of Normal Individuals and of Psychopathic Stammerers

Abscissa: Hydrogen ion concentration in terms of pH
Ordinate: Relative frequency in terms of per cent
———————— indicates normal individuals
— — — — — — indicates psychopathic stammerers

A series of determinations similar to those just described as having been made on sub-breathers was applied to 10 distinctly psychopathic stammerers, embracing 50 determinations distributed thus: 2 determinations each on 2 subjects, and 6 determinations each on 8 subjects. The results are presented in Table 4 and in Chart 2.

Table 4 and the accompanying Chart 2 show clearly that the hyperexcitable psychopathic stammerers examined had a much lower hydrogen ion concentration and carbon dioxide content of the mixed saliva than have normal healthy individuals. With the exception of one psychopathic sub-breather, whose salivary pH was on one occasion 6.90 and on another 6.95, every specimen examined was between pH 7.15 and 8.00. The mean, mode, and median were respectively 7.70, 7.55 to 7.75, and 7.60. Eighty per cent of the specimens were above pH 7.25 and below 7.95. It appears evident, therefore, that the psychopathic stammerer is in general so hyperexcitable that his emotional balance, such as it is, is upset by the mere act of furnishing a specimen of saliva, or that he is in a state of chronic excitement. Probably both conclusions are true. For whether apparently upset or not, the psychopath's salivary pH was almost invariably far above the normal limits, and even exceeding those found obtaining in normal individuals when intensely excited. That the majority of these subjects were abnormally perturbed by the mere presentation to them of the saliva collection tube was indicated by their turning away their heads, flushing, etc. This behavior was in marked contrast to the usually more or less dull and sodden "spitting" of the typical sub-breather. These findings, then, would also indicate that a persistently high salivary pH is an index of more or less chronic emotional excitement.

3. *Do hyper-excitable subjects show evidence of their excitement by concomitant decrease in hydrogen ion concentration and carbon dioxide content of the mixed saliva, and may these salivary biochemical characteristics be employed as indices of transient emotional excitement?*

The sub-breathing and psychopathic stammerers employed in the two preceding investigations were examined in the following manner. Each subject, after the ejection of a mouthful of saliva (the pH and carbon dioxide content of which were determined as before described), was excited as far as possible by verbal goading of a stereotyped form. This goading was not such as would be offensive to a normal individual, but was quite enough to disturb a hyper-excitable or psychopathic subject. It was continued for 5 min., when the collection tube was again presented to the subject, and he was directed to eject into it all of the saliva now in his mouth. (In four in-

TABLE 5. The Effect of Verbal Goading upon the Ability to Speak and the Hydrogen Ion Concentration of the Mixed Saliva of Sub-breathing Stammerers

	Before goading					After 5 min. goading				After 10 min. rest period		
No.	Salivary pH	Ability to speak within 1′	2′	Ability to speak within 1′	2′	Salivary pH	Diff. from initial pH	Ability to speak within 1′	2′	Salivary pH	Diff. from pH during goading	Diff. from initial pH
1	5.40	−	+	−	+	5.40	0.00	−	+	5.40	0.00	0.00
2	5.50	−	+	−	+	5.55	+ 0.05	−	+	5.55	0.00	+ 0.05
3	5.60	−	+	−	−	5.75	+ 0.15	−	+	5.65	− 0.10	+ 0.05
4	5.65	+		+		5.60	− 0.05	+		5.60	0.00	− 0.05
5	5.75	+		−	+	5.80	+ 0.05	−	+	5.70	− 0.10	− 0.05
6	5.75	+		−	+	5.65	− 0.10	+		5.60	− 0.05	− 0.15
7	5.80	+		+		5.85	+ 0.05	+		5.85	0.00	+ 0.05
8	5.85	−	+	−	−	6.00	+ 0.15	+		5.80	− 0.20	− 0.05
9	5.85	+		+		5.85	0.00	−	+	5.85	0.00	0.00
10	5.85	−	+	−	+	5.90	+ 0.05	−	+	5.90	0.00	+ 0.05
11	5.90	−	−	−	+	6.00	+ 0.10	−	+	5.90	− 0.10	0.00
12	5.90	−	+	−	+	5.90	0.00	+		5.90	0.00	0.00
13	5.90	+		+		5.95	+ 0.05	+		5.95	0.00	+ 0.05
14	5.90	+		+		5.90	0.00	−	+	5.85	− 0.05	− 0.05
15	5.95	+		−	−	6.15	+ 0.20	−	+	5.95	− 0.20	0.00
16	6.00	−	+	−	−	6.20	+ 0.20	−	+	6.05	− 0.15	+ 0.05
17	6.00	+		+		6.00	0.00	−	+	6.00	0.00	0.00
18	6.00	+		+		6.05	+ 0.05	+		6.05	0.00	+ 0.05
19	6.20	−	+	−	+	6.20	0.00	−	+	6.20	0.00	0.00
20	6.20	−	+	−	−	6.45	+ 0.25	−	+	6.25	− 0.20	+ 0.05
21	6.25	−	+	−	+	6.20	− 0.05	+		6.20	0.00	− 0.05
22	6.30	−	+	−	+	6.30	0.00	+		6.30	0.00	0.00
23	6.30	+		+		6.25	− 0.05	+		6.25	0.00	− 0.05
24	6.35	+		+		6.40	+ 0.05	−	+	6.35	− 0.05	0.00
25	6.40	+		−	+	6.50	+ 0.10	+		6.35	− 0.15	− 0.05
26	6.50	−	+	+		6.50	0.00	+		6.50	0.00	0.00
27	6.60	−	+	−	−	6.90	+ 0.30	+		6.50	− 0.40	− 0.10
Total	161.95					Total 163.20				Total 163.25		
	27)161.95					27)163.20				27)163.25		
average	6.00					average 6.04				average 6.05		

stances, when the mouth was dry, the subject was urged to expectorate as soon as possible, and the required 1 cubic cm. was forthcoming within 1.5 min.) The subject was then immediately ordered to say "yes, sir," and his ability to say it within 1 or 2 min. was carefully noted, as being largely indicative of the extent of his emotional disturbance. The saliva ejected immediately after the goading was examined in the same manner as the former specimen, and the subject was allowed a 10 min. period of quietude during which to calm down. Then, after a few remarks, calculated to prevent excitement as far as possible, he was again required to say "yes, sir," the time consumed being again noted, and his saliva was again examined as before.

The experiment was conducted on 37 subjects, of whom 27 were decided sub-breathers and 10 unquestionably psychopathic. In no instance do the data presented include more than one investigation of the same individual. For it is obvious that, having once experienced the goading and ascertained its purpose, subsequent repetition of the experiment upon a subject would not have had an effect comparable with that upon one not thus initiated.[23] The results obtained are presented interpretatively in Tables 5 and 6.

SUMMARY OF TABLE 5. Sub-breathers.

Average initial hydrogen ion concentration of the mixed saliva = pH 6.00
After 5 min. verbal goading:
- Average hydrogen ion concentration of the mixed saliva = 6.04.
- 14.8% of the subjects showed a slight decrease in salivary pH, ranging from 0.05 to 0.10, and averaging 0.06.
- 29.6% of the subjects showed no change in salivary pH.
- 55.6% showed a slight increase, ranging from 0.05 to 0.30, and averaging 0.12.

After 10 min. period of quietude:
- Average hydrogen ion concentration of the mixed saliva = 6.05.

Difference from salivary pH immediately after goading:
- 55.6% of the subjects showed no change in salivary pH.
- 44.4% of the subjects showed a slight decrease, ranging from 0.05 to 0.40, and averaging 0.15.
- Average change in salivary pH for the group was minus 0.065.

Difference from initial salivary pH:
- 33.33% of the subjects showed a slight decrease, ranging from 0.05 to 0.15, and averaging 0.067.
- 37.04% of the subjects showed no change.
- 29.63% of the subjects showed a slight increase, which in every instance was 0.05.
- Average change in salivary pH for the group was minus 0.007.

[23] In a subsequent investigation we expect to make a series of similar examinations of the degree of emotional stability or instability, expressed in terms of change of salivary pH due to an emotionalizing stimulus comparable to the initial verbal goading, of the same subjects when they are ready for discharge as cured from the Clinic, to determine the stabilizing value of the corrective treatment they have received.

TABLE 6. The Effect of Verbal Goading upon the Ability to Speak and the Hydrogen Ion Concentration of the Mixed Saliva of Psychopathic Stammerers

	Before goading					After 5 min. goading				After 10 min. rest period		
No.	Salivary pH	Ability to speak within 1′	Ability to speak within 2′	Ability to speak within 1′	Ability to speak within 2′	Salivary pH	Diff. from initial salivary pH	Ability to speak within 1′	Ability to speak within 2′	Salivary pH	Diff. from pH during goading	Diff. from initial salivary pH
1	6.80	—	+	—	—	7.15	+ 0.35	—	+	6.90	— 0.25	+ 0.10
2	7.30	—	—	—	—	8.10	+ 0.80	—	—	7.80	— 0.30	+ 0.50
3	7.50	+		—	+	7.70	+ 0.20	—	+	7.55	— 0.15	+ 0.05
4	7.50	—	+	—	—	8.00	+ 0.50	—	+	7.60	— 0.40	+ 0.10
5	7.65	—	+	—	—	8.00	+ 0.35	—	+	7.70	— 0.30	+ 0.05
6	7.65	+		—	+	7.75	+ 0.10	+		7.60	— 0.15	— 0.05
7	7.70	—	+	—	—	8.10	+ 0.40	—	+	7.60	— 0.50	— 0.10
8	7.50	—	+	—	—	7.95	+ 0.45	—	—	7.80	— 0.15	+ 0.30
9	7.35	—	+	—	—	7.65	+ 0.30	+		7.35	— 0.30	+ 0.00
10	7.40	+		—	—	7.95	+ 0.55	—	—	7.75	— 0.20	+ 0.35
Total	74.35					78.39				75.62		
	10)74.35					10)78.39				10)75.62		
Average	7.44					7.84				7.56		

SUMMARY OF TABLE 6. Psychopaths.
Average initial hydrogen ion concentration of the mixed saliva = 7.44.
After 5 min. verbal goading:
Average hydrogen ion concentration of the mixed saliva = 7.84.
100% of the subjects showed an increase in salivary pH, ranging from 0.10 to 0.80, and averaging 0.40.
After 10 min. period of quietude:
Average hydrogen ion concentration of the mixed saliva = 7.56.
Difference from salivary pH immediately after goading:
100% of the subjects showed a decided decrease, ranging from 0.15 to 0.50, with an average of 0.27.
Difference from initial salivary pH:
20% of the subjects showed a slight decrease, averaging 0.075.
10% of the subjects showed the same salivary pH as before the goading.
70% of the subjects showed a higher salivary pH than before the goading, the increase ranging from 0.05 to 0.50, and averaging 0.21.
Average change in pH for the group = plus 0.13.

(*a*) The first point that we notice in considering the data presented in Tables 5 and 6 is that the average initial salivary pH of the sub-breathers is 6.0 and that of the psychopaths 7.4, indicating the same fact made evident in Tables 3 and 4, *i. e.*, that the sub-breathers show a salivary pH in general considerably below, and the psychopaths display one considerably above, that of the normal individual (approximately 6.8).

(*b*) The next striking differentiation to be noted is that as a group the sub-breathers showed practically no change in salivary pH as the result of the verbal goading. (The average change for the group was only + 0.057.) A change of less than 0.10 in salivary pH may be regarded as negligible. The few subjects who did show noticeable change—especially nos. 20 and 27, whose salivary pH increased respectively 0.25 and 0.30—displayed hyper-excitability and more or less decided neurotic tendencies to such a degree that at the outset it was difficult to decide whether to list them with the sub-breathers or the psychopaths. It will be noted that they were both totally unable to say "yes, sir" within 2 min. after the cessation of the goading. Subjects 15 and 16, showing an increase in salivary pH of 0.20, were not psychopathic, but were hyperexcitable. Both displayed a somewhat exaggerated patellar reflex (as did also nos. 20 and 27, to an even more marked degree). Neither was able to say "yes, sir" within the stipulated 2 min. The sudden opening of a door behind him in the office where he was seated, presumably comfortably, for the collection of a specimen of saliva, caused no. 16 to start perceptibly.

The psychopaths in general showed a pronounced increase in salivary pH following the verbal goading. In every instance there was an increase ranging from 0.10 to 0.80, with mode and

median approximately 0.4 and a mean of exactly 0.4. That a high degree of excitement resulted from the goading was amply demonstrated by the behavior of the psychopaths. All except nos. 3 and 6 (who showed an increase in salivary pH respectively of only 0.20 and 0.10) were unable to respond to the command to speak with more than voiceless twitching of the lips for more than 2 min. No. 6 forced out the required "yes, sir" in 1′ 22″ accompanying it with a determined shake of the head.

It is thus quite evident that, both as regards the psychopaths and the sub-breathers, emotional disturbance and increase in salivary pH went *pari passu.*

(*c*) The third differentiation brought out by this investigation was with regard to decrease in salivary pH upon subsidence of the emotional excitement. After the 10 min. period of quietude, all of the sub-breathers succeeded in enunciating "yes, sir" within the required 2 mins., with the exception of no. 27, who took 2′ 28″. Nos. 20 and 26 succeeded in the test barely within 2 min. Each one of the three subjects had displayed under goading a rather high degree of hyper-excitability, indicated by marked rise in salivary pH and concomitant decrease in carbon dioxide content of the saliva. In connection with this ability to speak after the period of quietude, noted in the majority of the sub-breathers, it may be remarked that the final salivary pH of each of them, except no. 27, was practically identical with his initial salivary pH.

The psychopaths, however, varied greatly among themselves as to their ability to pronounce "yes, sir" after the 10 min. period of quietude. It required 4′ 18″ for no. 2 to get it out, which might have been anticipated from the fact that his final salivary pH was 0.5 higher than his initial one. Subject 6 responded with the test words almost immediately when requested to do so. Under goading his salivary pH had increased only 0.10, and after the rest period it was practically the same as at the beginning of the examination. Nos. 8 and 10 were unable to speak within the 2 min. limit. The salivary pH of each at this time was still respectively 0.30 and 0.35 above the initial pH. No. 9 required 59″ to get out the test words. The others required more than 1′ and less than 2′. It is evident, therefore, that the psychopathic stammerers not only become more easily excited than do the sub-breathers, but that in the main their excitement subsides more slowly,—or is reestablished much more readily. For it is difficult to say definitely whether the psychopaths, whose final salivary pH was above their initial pH, were *still* excited or *again* excited.

It would appear from these tests that the hydrogen ion concentration of the mixed saliva, when its fluctuations are due to variations in the quantity of carbon dioxide present, as was the

case in every one of the foregoing analyses, may be employed to determine the relative excitability of an individual.

Application.—The tests already described were applied to a group of 7 stammerers who showed little if any anatomical indication of defective breathing and who were not psychopathic. The results are presented in Table 7.

TABLE 7. Examination of Seven Unclassified Stammerers

	Before goading					After 5 min. goading				After 10 min. rest period		
No.	Salivary pH	Ability to speak within 1′	2′	Ability to speak within 1′	2′	Salivary pH	Diff. from initial PH	Ability to speak within 1′	2′	Salivary pH	Diff. from pH under goading	Diff. from initial pH
1	6.10	+		+		6.15	+ 0.05	+		6.15	0.00	0.05
2	6.30	—	+	—	+	6.25	— 0.05	+		6.25	0.00	—0.05
3	6.85	+		+		6.90	+ 0.05	+		6.85	— 0.05	0.00
4	6.95	+		+		6.90	— 0.05	+		6.95	+ 0.05	0.00
5	6.95	—	+	—	—	7.35	+ 0.40	—	—	7.15	— 0.20	+0.20
6	7.00	—	+	—	+	7.30	+ 0.30	—	+	7.10	— 0.20	+0.10
7	7.35	—	+			7.65	+ 0.30	—	+	7.40	— 0.25	+0.05

Interpreting Table 7 in the light of the preceding findings we may diagnose nos. 1 and 2 as not hyper-excitable sub-breathers; nos. 3 and 4 as neither sub-breathers, psychopathic, nor hyper-excitable (no. 4 was, in fact, an old case about ready for dismissal from the Speech Clinic as Cured); nos. 5 and 6 as hyper-excitable; and no. 7 as hyper-excitable and probably more or less psychopathic.

SUMMARY

As a result of this research, involving about 1300 salivary analyses and psychological diagnostic judgments, what has been found?

First, as to stammerers. There is one group of stammerers, embracing 73.7 per cent of the stammerers examined in the general survey of those who applied for aid to the Speech Clinic of this University during the scholastic year of 1921-22, who may be denominated as sub-breathers and who have their organisms overloaded with carbon dioxide. As a consequence their mental faculties are dulled, they are always working in a "fatigued" condition, virtually under pressure. Until the carbon dioxide content of their blood has been reduced more nearly to normal limits it is practically hopeless for them to attempt to break any old habits or acquire any new ones, whether of speech or otherwise. Proper breathing exercises in the open air adapted to the individual's requirements should be of immense value in this connection.[34] A decrease in the carbohydrate content of the diet should also prove of benefit.

[34] A series of experiments is now in progress to determine the efficacy in freeing the system of excess carbon dioxide by means of certain specific breathing exercises.

Another, and considerably smaller group of stammerers are distinctly psychopathic,—somewhat less than 15.4% of those examined in the present research. These subjects are generally very hyper-excitable. They discharge a disproportionate amount of energy in response to an immediate stimulus, displaying no sense of proportion. A stimulus which would have very little effect upon a normal subject, and none at all upon a typically dulled sub-breather, upsets the "equilibrium" of a psychopath completely. They are practically hopeless subjects for remedial measures, so far as correction of their speech defect is concerned, unless their general psychopathic condition is first cured. For while they may respond with all their power to the stimulus of treatment, in the way of exercises, drill, etc., in the presence of the teacher, there is little if any apperceptive residuum left. Once out of sight of the Clinic, what they have there done and been told to do is speedily eradicated from their minds by fresh stimuli, to which they respond with the same disproportionate display of energy.

Obviously another type of stammerer may be both hyper-excitably psychopathic and a sub-breather. Such a subject in a hyper-excited condition might eject a saliva apparently normal as to pH, inasmuch as his sub-breathing habits would tend to keep his salivary pH low, while his hyper-excited condition would tend to raise it. Between the two contending factors, the salivary pH, at some given moment when the specimen is collected and the determination made, may be occupying a median position approximating that of a normal saliva. An adequate series of determinations, however, should show his predominant tendency, and the series of tests with and without verbal goading will serve to indicate his degree of excitability.

A fourth group of stammerers may not be dulled by defective breathing or other cause of overloading the system with carbon dioxide, resulting in chronic "fatigue" and concomitant lack of energy, nor may they be psychopathic. They may simply be hyper-excitable. Perhaps it is from this class that the psychoanalyst recruits his subjects.

In the light of our findings as to the metabolic etiology of stammering, we may sum up the therapeutic measures indicated by stating that the sub-breathers must be toned up and their systems freed from excessive carbon dioxide by a technique probably involving vigorous breathing exercises and a control of the diet, in addition to the regular drill; the hyper-excitables must be calmed down; and the psychopaths—sent first to a psychiatrist.

As to the hydrogen ion concentration of the mixed saliva in conjunction with determinations of the carbon dioxide content, the present research has found it useful as an index of

(1) the condition of an individual as to fatigue or energy at his disposal, and the breathing habits of the individual;

(2) the degree of emotional excitement under which the subject is laboring at the time of the determination—thus enabling the constantly excited psychopath to be readily detected; and

(3) the degree of excitability of an individual, by means of a series of determinations made before, during and after the application of a definite emotionalizing stimulus.

The question remains why the hydrogen ion concentration of the mixed saliva, due to the presence of carbon dioxide, is abnormally high in sub-breathers and fatigued individuals, and abnormally low in chronically excited psychopaths. And why in excitement does the salivary pH tend to rise in both normal and abnormal individuals? No dogmatic answer may be given. It would appear, however, that the explanation is somewhat as follows. As a result of either sub-breathing, muscular exertion, or excessive ingestion of carbohydrates, carbon dioxide accumulates in the blood and in the alveolar air in excess of the normal tension. The result is, *normally*, a certain degree of hyperpnoea to relieve the tension, for carbon dioxide is a specific stimulus of the respiratory center as are also lactic acid and other products of fatigue,[35] the teleology of the hyperpnoea being to expel enough carbon dioxide so that normal limits may again be reached. The consequence is that normally the carbon dioxide content of the alveolar air varies but little in an individual, an increase in carbon dioxide content of the blood resulting in augmented respiration rather than in accumulation of carbon dioxide in the alveolar air. There is, however, some increase in the latter, and, as we have found, the salivary hydrion concentration increases as the carbon dioxide content of the alveolar air increases, but to a greater degree. Consequently, a high hydrion concentration of the saliva, when due to carbon dioxide, indicates a high carbon dioxide content of the alveolar air, and therefore of the blood. A high carbon dioxide content in the blood manifests itself in fatigue, dullness, inertia.[36] This concomitant excitation of the respiratory center and dulling of the centers stimulating the individual to activity has probably developed ontogenetically because of its phylogenetic import. For when there is a great accumulation of carbon dioxide throughout the organism, further activity might endanger life, and the individual would probably keep going until he inadvertently committed suicide. But fatigue, inertia, dullness, lead him to a cessation of carbon dioxide producing effort, and meanwhile the stimulation of the respiratory center, as noted, normally leads to expulsion of the excess carbon dioxide, and a return to normal conditions of the blood and system in general. It is possible in the case of the most pronounced sub-breathing stammerers, or other individuals with markedly high salivary hydrion concentrations due to the presence of carbon dioxide in the saliva, that in addition to, or in the absence of, pernicious breathing, working, or dietary habits *per se*, there may be a lack of sensitivity of the respiratory center, so that the carbon dioxide may accumulate in the blood leading to a chronic fatigue, or even somewhat of a mental numbness, without the normally concomitant hyperpnoea to rid the system of the excess carbon dioxide. In this event, also, very vigorous breathing, deliberately and regularly practised (a sort of artificial hyperpnoea) may be of great remedial value.

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[35] Cf. Robertson, T. B., Principles of Biochemistry. N. Y. and Phila., 1920. Pp. 364-368.

[36] For the toxicology of carbon dioxide see Kobert, R., Lehrbuch der Intoxikationen, II. Stuttgart, 1906. Section on Kohlensäure, pp. 1120-23.

With regard to the problem why the psychopaths examined showed constantly such abnormally low hydrogen ion concentration of the mixed saliva (and consequent high pH), we must again avoid dogmatic assertion. Their practically chronic emotional excitement, whether or not displayed by overt acts, suggests that the same physiological etiology obtains as in the case of any subjects, normal or abnormal when their salivary pH increases during emotional stress. This may be attributed largely to the hyperpnoea, amounting in high degrees of excitement to a veritable dyspnoea, which rids the alveolar air (and consequently the blood) of a large part of its carbon dioxide content. In psychopathic subjects the respiratory center *may* be abnormally sensitive to relatively small quantities of carbon dioxide in the blood, or there *may* be abnormal activity of the adrenals resulting in excessive quantities of adrenin in the blood, causing a more or less chronic dilatation of the bronchioles and thereby diminished carbon dioxide tension of the blood and alveolar air. In either or both cases the result would be decreased carbon dioxide content of the mixed saliva and consequent lower hydrogen ion concentration, indicated by increased salivary pH.

In normal individuals the functioning of the adrenals in times of emotional stress is probably a prime factor in the stimulation of the respiratory center and dilatation of the bronchioles. In other words, in emotional excitement the functioning of the adrenals prepares the organism for flight or fight by ridding the system to a greater degree than usual of carbon dioxide, and preparing it for the speedy removal of the excessive quantities of carbon dioxide which would result from intense exertion. The teleology of the function is the prevention or postponement of fatigue and exhaustion. When the prepared-for exertion does not follow, the carbon dioxide content of the blood and of the alveolar air probably remains for some time below the normal limits, with the result that there is an even more greatly diminished carbonic acid content and consequently lowered hydrogen ion concentration of the mixed saliva, indicated by a high salivary pH.

LAUGHTER, A GLORY IN SANITY

By Ransom Carpenter

In offering a theory of laughter to the judgment of psychologists I must begin by explaining that I am not myself a psychologist, and that in preparing the matter here following I have necessarily written as if addressing readers of my own nontechnical level.

Without knowing (or possibly having forgotten) that the nature of the comic was a favorite riddle of philosophers, I reached an answer that satisfied my own curiosity, and later went to a library to consult the authorities. My impression grew, the more I read, that in the attempt to explain so universal, familiar, and open a phenomenon as laughter there was a tendency to resort to solutions either limited in scope or abstruse and indirect in their application. The true answer, I thought, must needs be as broad and simple as laughter itself; yet the exertions of printed philosophy seemed to display an unaccountable strain toward the narrow and complex.

That there are manifold mental complexities underlying and surrounding the act of laughter I do not question. That field remains for better equipped explorers. I simply place on view a bit of mental mechanism which, when once pointed out, I believe brief introspection will identify to any thinker as the main actuating principle of laughter, but which for some mysterious reason has eluded the notice of other inquirers.

Indeed I cannot suppose that no one else has formulated, at least to himself, so simple a principle. I only know that a fairly careful search of available publications,[1] including recent copious works that review the prior field exhaustively, has failed to disclose a single line that definitely anticipates my explanation.

Now I shall try to state, in my own way, and so far as possible without reference to any preceding theory, what it seems to me that laughter is. Laughter is the outcry of the soul exulting in sanity. Or, more soberly and completely, laughter expresses an emotion due to a sudden flooding into consciousness of the subconsciously abiding pleasure in the power of judgment, occasioned by the swift overthrow of presented propositions that tend but fail to delude the judgment.

The possession of reason is obviously good cause for continuous elation, and we do in fact rejoice in it unceasingly; it is part of that basic joy for which people cling to life despite any miseries. But our realization of it, like that of other glories, is glossed over with practical habit. We are used to it. So long as the path of thought is plain and even, we walk in it soberly, exercising our cherished judgment as a matter of course, accounting unconsciously for the multitude of facts or ideas presented, finding them in order, coherent with sanity. But let a pitfall of absurdity appear in the

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[1] Schopenhauer, A., The World as Will and Idea, iii, VIII, On the Theory of the Ludicrous.

Bergson, H. L., Laughter; an Essay on the Meaning of the Comic; Brereton and Rothwell's trans., 1911.

Sully, James, An Essay on Laughter, 1902.

Freud, Sigmund, Wit and Its Relation to the Unconscious; Brill's trans., 1916.

Sidis, Boris, The Psychology of Laughter, 1913.

Bliss, Sylvia, The Origin of Laughter, this Journal, xxvi, 1915, 236ff.

Eastman, Max, The Sense of Humor, 1921.

pathway, so that the mind at the same moment sees where it might have slipped yet walks erect; then the sense of sanity swells abruptly into sharp emotion that is voiced in laughter.

Why a glory in sanity should find expression in spasmodic noises is, of course, a question of physiology, with an answer lying somewhere in the long history of bodily evolution. It does not concern my purpose, which is to show what laughter is within us, the nature of the comic sense.

To make the matter clearer let us examine, in the light of this explanation, the nature of the things and ideas that provoke laughter. Why, let us ask, is one notion funnier than another?

To be comic, a proposition must be (1) perceived as false, and (2) perceived as deceptive. These are the essentials. Actually to excite laughter, it must generally also (3) be suddenly presented, and (4) have a free field, in which its effect is not submerged by stronger emotions.

An idea is most comic when all these factors are most favorable. The height of the ludicrous is reached by what is wild, yet plausible. The wilder and more plausible the rejected idea, the more sharply it throws open the valve that lets out a gust of elation at reason's triumph. The virtue of a moderately good joke may lie either in an extreme falsity with a slightly deceptive element, or in an approach to truth so near that it almost convinces. The two elements, of course, may both be of moderate force, or one or the other may dwindle within the realm of bad jokes to the vanishing point, leaving accepted truth or bare falsehood, neither of which is funny.

In one of the treatises that I examined[8] there is an inconclusive discussion of a previous writer's effort to penetrate the mystery of the comic aspect of a child wearing his father's hat. This example will serve as well as any for first pointing out the virtues of my solvent. We smile at this mildly funny sight because the child's pretense offers to our mind the obviously false proposition, "This hat might make you think me a man," and there is just enough plausibility in the idea for the act of rejecting it to remind us faintly of the fact that we are sane. That is all there is to the mystery.

Another favorite query of investigators is why a grotesque mask is funny. Here the falsity presented may be expressed as "There might be such a person as this," or "People are like this," and the deceptive element is obvious. The spectator's mind is put on the alert against any yielding, perhaps momentary or half conscious, to the offered illusion, and the pleasure which the ego takes in this resistance is nothing more or less than the sense of the comic.

Again, the writers often ask why we should laugh at the fall of one who slips or stumbles. Some even gloomily attribute to laughter, on this ground, a cruel or debasing element. Nothing could be more opposed to truth. If the fall is really cruel, the normal person does not laugh. A comic fall is one that presents a delusive aspect of catastrophe, which we instantly recognize as unreal, relishing the recognition. The more "scary" the fall, the greater is the exertion of judgment and the more we exult. As for the abnormal person who laughs at another's pain or misery, his subconscious point of view may be thus expanded: "That old woman's fall might be supposed to excite pity, but being superior to such weak sentiments I am not deluded." So he triumphs in his own peculiar kind of sanity.

A child's laughter should offer clarifying examples. I remember being startled, long before I had thought the matter out, by observing how my daughter, then less than two years old, laughed most merrily at just those antics of a playful kitten that I myself found most diverting. Now I see that within the scope of the child's experience she had as good a right as I to triumph in the exercise of judgment, and more reason, because judg-

[8]Sully, *op. cit.*, 9-17.

ment was for her, so to speak, a newer plaything. The kitten by its capers repeatedly suggested that it was all the same as a real person, which the baby knew perfectly well was not true. The kitten's best moments as a comedian were those in which it most plausibly mimicked some human trait or purpose. If the impersonation had at any point become convincing, the baby would not have laughed. She might rather have been frightened out of her wits. But being well in possession of her wits, and using them vigorously to combat the kitten's wild assertions, she naturally rejoiced aloud.

A child who thus takes pleasure in thrusting off persuasive untruths soon learns to build up fictions for the fun of demolishing them. So we have play, which Sully considered almost identical with mirth. The true relation will now be manifest. Laughter is by no means always derived from play, but only when the plausible falsity that provokes it is framed with intention to create amusement. The pleasure of play (so far as it is "make-believe") resides precisely in our continuous perception of its unreality, and we enhance the realism of our play only so as to gloat the more in that perception.

One of the sources of the comic before which the cloud of mystery seems to have hung with a peculiar allurement may best be indicated by the general label "breaches of decorum." Analysis of one innocent example will serve to sweep away the fog from all, without, let us hope, dispelling any of their charm. Let us consider a man who snores in church, and why he is funny.

The atmosphere of a church may be put in the form of an assertion: "This is a solemn place where everyone is always quiet and attentive." The incongruous snore abruptly compels rejection of this proposition as merely plausible, at the same time offering the counter suggestion, "This man owes no reverence," which the mind as promptly dismisses because the disturbance is unintended and unimportant. Thus the ego's triumph lies partly in detecting the flaw in the church's veil of solemnity, and partly in perceiving the inconsequence of the defect.

The reader with a particular interest in this direction will find it easy to extend the principle to profanities, indecencies, tribal taboos, sexual lapses, and similar items the discussion of which has tended to impart a mystic air to anthropology. In every instance of the kind judgment will be found striking a balance between opposed pretensions of "decorum" or law, on the one hand, and the "breach" or liberty, on the other, and glorying in the sane escape from both.

In the few foregoing instances, I have tried to reduce the material to the simplest possible terms, to exhibit the working of the formula. Indeed the barest outline will suffice to diagram a multitude of jokes, including many good ones. But the laughable proposition may be double or complex, and generally it wears a fringe of implied comicalities, vague or subtle perhaps, but always enriching. Humor, I suppose, resides in such broad and indefinite implications, often conveyed by statements in no way comic of themselves, whereas wit is sharp, simple, and direct in outline. For illustration, let us glance at this yarn from a random newspaper:

"There is a philosophic old colored barber in Washington who is much patronized by statesmen. They find much that is edifying in his conversation.

"One day the old fellow, being in an especially talkative mood, made this observation to a newly arrived representative from the West:

" 'Yo' has a large head, suh. It's a good thing to have a large head, fo' a large head means a large brain, an' a large brain is de most useful thing a man kin have, fo' it nourishes de roots of de hair.' "

The mainspring of the joke is in the last twenty words, which by themselves form a good single specimen of wit, assuming they were intended playfully. Yet they convey at least two main delusive falsehoods, the

instant double overthrow of which reassures us of sanity and unlooses elation. One may be put as "Brains nourish the roots of the hair;" the other as "The use of brains is to grow hair." But our laughter is the keener because of the beautifully laid approach to the pitfall, which remains invisible up to the very last words. The effect is also richer for a variety of comic elements that are only suggested or implied, involving, let us say, the known peculiarities of congressmen and of barbers, perhaps also of colored persons and of philosophers. Each of these elements could readily be dissected out and placed in the form of a plausible fallacy, contributing by its downfall to the glow of reason's satisfaction.

One is tempted to speculate on the social value, if any, of knowing what laughter is. I think it truly matters, but here a word in closing must suffice. I have mentioned how some writers have found cruelty and malice lurking in the happy faculty, and this is not the only charge laid against it. Hobbes' definition[3], so apt in the phrase "sudden glory," has yet played a most unfortunate part in misleading later analysts, through the reference to "comparison with the infirmity of others" as the spring of laughter. Because Hobbes failed to perceive that in mirth the mind glories in its own successes, by comparison with nothing but its own avoided errors, a long train of thinkers have tended to look on laughter as implying vanity, arrogance, or selfishness. In point of fact, even when laughter seems to express malice or contempt, it is not the true voice of those emotions, but always of some residual shred of sympathy. Scorn may be expressed without sympathy and without laughter; if laughter enters, it can only mean that the laugher perceives how he might have shared the weakness that he contemns.

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[3]Though frequently quoted, repeated here for comparison: "The passion of laughter is nothing else but sudden glory arising from a sudden conception of some eminency in ourselves, by comparison with the infirmity of others, or with our own formerly."

A NOTE ON HENNING'S SMELL SERIES

By Forrest L. Dimmick, Univ. of Michigan

Henning[1] has given us a thorough discussion of his researches in smell so far as their historical and theoretical aspects are concerned. As Gamble[2] has already pointed out, however, the details of experimentation are very meager. Henning's most striking contribution is, probably, his serial classification of odors; yet nowhere does one find listed the qualities of any considerable part of the 415 stimuli[3] used. At most there are examples of several of the series from the reports that one or another *O* gave; but these examples vary greatly in completeness and, by Henning's own profession, are not unequivocal, but differ from time to time and from *O* to *O*. Anyone who has attempted to make a class-demonstration or an elementary laboratory experiment based on the new classification of odors has come upon these difficulties.

We have attempted to supply in some measure the data which Henning has omitted. Following the example of others[4] we have accepted the Henning prism as a starting-point. From the sample lists of the several series which Henning gives, we have taken all those stimuli that can be readily obtained, 75 in number. Small quantities of the substances were placed in two-drachm bottles which were numbered on the side and on the cork There was no marking on the bottles which could give the *Os* a cue to the nature of the contents, and the numbered labels practically concealed them.

The *Os* knew to start with the general nature of Henning's classification, and they had been given Henning's typical corner odors. Every *O* was required to make one complete classification of the entire set. He was instructed to "choose at random ten stimuli; to smell them one at a time with both nostrils; and to record every particular number under the group heading or headings that best described the odor." Not more than 10 stimuli were smelled at a sitting. The *Os* were, in the main, members of an advanced class which had just taken up the systematic study of olfactory sensation, and of an elementary laboratory class. A few well trained *Os* were included. A total of 16 classifications was made. The first classification made by every *O* is the one taken for our results, inasmuch as our purpose is to find a set of odors that will illustrate the series unequivocally and immediately.

In Table I we have brought together the results of the 16 classifications. In the second column, under H, the designations indicate the approximate classification of the odors according to Henning[5]. The numerical results show the distribution of the classifications made by our *Os*. The greatest number of choices that can be made under a single heading is 16, but a greater total number for a single stimulus may result from its classification as an intermediate odor.

[1]H. Henning, *Der Geruch*, 1916. Printed earlier in the *Zeit. f. Psychol.*, 73-76, 1915, 1916, in four parts.

[2]E. A. Mc. Gamble, this Journal, 32, 1921, 290.

[3]Henning, *op. cit.*, 7.

[4]Gamble, *op. cit.*; E. B. Titchener, this Journal, 31, 1920, 213.

[5]*Op. cit.*, 80-97.

TABLE I

Stim	H	Fl	Fr	Sp	Re	Bu	Fo	Stim.	H	Fl	Fr	Sp	Re	Bu	Fo
Vanilla	Fl	6	11	6	1	1	—	Acetic Ether	ReFr	3	6	—	9	3	2
Jasmine oil	Fl	15	1	—	—	—	—	Ethyl Ether	ReFr	5	5	—	8	2	—
Tonka Bean	Fl	8	—	9	—	—	—	Acetone	ReFr	2	6	2	9	3	1
Apple Blossom*		16	—	—	—	—	—	Collodium	ReFr	1	7	1	9	5	—
Camomile fl.	FlSp	5	—	14	—	2	—	Lemon o.	Fr	—	16	—	1	—	—
Lavender fl.	FlSp	6	6	12	1	—	—	Orange o.	Fr	5	14	—	1	—	—
Origanum o.	FlSp	1	4	3	14	5	—	Citronella o.	Fr	5	6	3	6	2	—
Arnica tin.	FlSp	1	11	2	3	9	—	Bergamot o.	Fr	—	3	4	11	—	3
Hops fl.	FlSp	1	—	8	1	2	9	Amyl-Valerate	Fr	—	15	—	1	—	—
Cumarin	FlSp	10	7	4	1	—	—	Geranium o.	FrFl	9	6	6	6	1	—
Caraway o.	FlSp	1	4	6	6	1	2	Pyridine	Bu	—	—	—	—	8	14
Clove o.	FlSp	3	4	12	2	—	—	Tar	Bu	—	—	—	—	14	2
Bay o.	FlSp	7	5	9	3	—	—	Coffee	BuSp	—	—	9	—	7	—
Thyme fl.	FlSp	3	—	13	—	—	—	Xylol	BuRe	1	1	1	11	6	—
Cassia o.	FlSp	3	3	14	—	—	—	Toluol	BuRe	1	—	1	11	5	2
Pepper	Sp	1	—	14	2	—	—	Benzol	BuRe	1	3	—	9	3	4
Sage	Sp	6	1	11	2	—	1	Fish Soap	Fo	—	—	—	1	3	15
Cinnamon	Sp	1	1	16	—	—	—	Glue	Fo	—	—	—	1	3	16
Ginger	Sp	8	6	5	6	—	—	H_2S†	Fo	—	—	—	1	—	15
Anise o.	SpRe	6	13	8	—	—	—	Ammon.-Valer.*		1	8	1	2	1	7
Marjoram	SpRe	3	—	11	2	—	—	Lactone	FoBuSpFl	4	1	5	2	—	4
Sassafras o.	SpRe	4	8	10	3	—	—	Apiol	FoBuSpFl	2	3	5	5	9	—
Nutmeg o.	SpRe	1	3	12	2	—	2	Asafoetida	FoBuSpFl	—	—	—	1	1	15
Fennel o.	SpRe	4	8	7	7	—	—	Celery Seed	FoBuSpFl	2	2	12	1	—	—
Cardamom	SpRe	2	4	10	8	1	—	Dill Seed	FoBuSpFl	3	1	13	2	—	—
Canada Bal.	Re	2	1	—	11	1	1	Mustard oil	FoBuSpFl	—	3	6	4	—	10
Cedar o.	Re	1	1	4	12	2	1	Guiacol	FoBuSpFl	2	—	—	2	14	—
Juniper o.	Re	—	4	3	11	1	—	Musk Root e.	FoBuReFr	10	—	2	2	4	2
Pine Need. e.	Re	—	1	4	12	—	—	Musk	FoBuReFr	1	5	6	5	6	2
Sandarak g.	Re	2	3	1	9	—	1	Arbor Vitae e.	FlFrSpRe	2	6	4	7	3	—
Spikenard o.	Re	—	2	—	14	2	—	Juniper Ber.	FlFrSpRe	3	3	7	3	1	—
Camphor g.	Re	—	1	5	10	—	—	Tansy o.	FlFrSpRe	3	—	8	9	5	—
Myrrh tin.	Re	4	6	4	3	5	—	Wormwood o.	FlFrSpRe	2	1	8	6	5	1
Turpentine	Re	1	—	—	15	2	—	Peppermint o.	FlFrSpRe	3	6	10	6	—	—
Cajuput o.	Re	—	2	4	13	—	1	Spearmint o.	FlFrSpRe	4	9	6	4	—	1
Rosemary o.	Re	—	4	5	12	—	—	Menthol	FlFrSpRe	—	5	10	3	3	1
Eucalyptus o.	Re	—	1	4	12	—	4	Amyl-acetate*		2	16	—	—	—	4
Lavender o.	Re	3	5	4	9	—	—								
Copaiba Bal.	Re	5	2	9	4	1	—								

Fl = flowery; Fr = fruity; Sp = spicy; Re = resinous; Bu = burned; Fo = foul; fl. = flowers; o. = oil; g. = gum; tin. = tincture; e. = extract; Bal. balsam; *not classified by Henning; † add a few drops of Hydrochloric Acid to Ammonium Sulphide.

It is evident from the table that valid examples of corner and intermediate odors can not be taken at random from the sample series of Henning. For example *vanilla*, which Henning gives as characteristically flowery, is designated more frequently by our *Os* as fruity; *tonka-bean* is as spicy as it is flowery; while *jasmine oil* is always flowery. There are many similar cases, especially among the intermediate odors. This same multiplicity of similarities is also remarked by Henning[6], and indeed is the basis of his conception of the Smell Prism as a surface figure[7].

Adequately to demonstrate the odor series, then, it is obviously necessary to select those odors which will fall most readily into their proper classes. We have attempted to do this in Table II. In it we have included under the proper headings (1) those stimuli which in at least 70% of the cases our *Os* classify in the same way as Henning, (2) in parentheses those stimuli in whose cases our *Os* do not agree so well with Henning, but are still not at total variance with him, and (3) in italics those stimuli in whose cases our *Os* totally disagree with Henning, but agree significantly among themselves.

TABLE II

Flowery	Fruity	Spicy	Resinous	Burned	Foul
Jasmine o. *Apple Blos.*	Lemon o. Orange o. Amyl-Valerate	Cinnamon Pepper Camomile f.	Spikenard o. Turpentine Cedar o. Pine-Needles Rosemary o. Eucalyptus o. Cajaput o.	Tar *Guiacol*	Fish Soap Glue H_2S *Asafoetida*
Fl-Sp	**Sp-Re**	**Re-Fr**	**Re-Bu**	**Fr-Fo**	**Fr-Fl**
Lavender f. Cumarin Cassia o. Clove o. Bay o. *Tonka-bean*	Marjoram Cardamom Nutmeg Sassafras o. Anise o.	Acetic Ether Ethyl Ether Acetone Collodium	Xylol Toluol	*Ammonium-Valerate*	*Orange o.* *Vanilla*
Bu-Sp	**Fo-Bu**	**Fl-Fo**	**Sp-Re-Bu**	**Fl-Fr-Fo**	
Coffee	*Pyridine*		*Tansy o.* *Wormwood o.*	*Amyl-acetate*	
Fl-Fr-Sp-Re	**Fl-Sp-Bu-Fo**	**Fo-Bu-Re-Fr**			
Arbor Vitae Juniper ber. Peppermint o. Spearmint o. *Myrrh*	(Apiol) (Lactone) (Mustard o.) *Hops f.*	*Bensol* (Musk)			

The foregoing table shows the stimuli which may be depended upon to demonstrate every corner, every edge (with one exception), and every surface of the smell prism. The stimuli, in the classification of which we agree with Henning, are to be preferred. Those in italics can be seen by reference to Table I to have been very positively placed by our *Os* and are therefore dependable. We have made no attempt as yet to arrange the intermediate odors serially between end-points. As Henning[8] remarks, a large number of intermediate steps is necessary to demonstrate adequately a psychological series. We hope to be able to duplicate and add to Henning's complete list of stimuli and to make out their serial arrangement.

[6] *Ibid.*, 97.
[7] *Ibid.*, 97.
[8] *Ibid.*, 500.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XLIII. Voluntarily Controlled Likes and Dislikes of Color Combinations

By Margaret Floy Washburn, Margaret T. MacDonald, and Dorothy Van Alstyne

In Vol. XXXII of this Journal, pp. 284 to 289, there was published a study from the Vassar laboratory which was concerned with the effects of instructing observers to try to alter their judgments of the pleasantness or unpleasantness of single colors. The colors were eighteen of the Bradley series, six each of saturated colors, shades, and tints, and were presented in the form of pieces 3 cm. square, each pasted in the middle of a card 2.5 by 3 in. "One of these cards was laid on a table before the observer, who was asked to express her judgment of the pleasantness of the color by using one of the numbers from 1 to 7, 1 meaning 'very unpleasant', 7 'very pleasant', and 4 'indifferent'. When the judgment had been expressed, the experimenter said, 'Now I want you to see if you can dislike that color,' if the judgment had been favorable; or, 'I want you to see if you can like that color,' if the judgment had been unfavorable. The original judgment and the altered judgment were recorded, and the observer was then asked how she had effected the change." About two months later each observer was asked to judge the pleasantness of the colors again. The results of this investigation will be mentioned in connection with those of the present study.

We have undertaken to repeat this procedure exactly, using color combinations instead of single colors.

Thirty-six cards 2.5 by 3 in. in dimensions were prepared. Each card had pasted on it two differently colored squares, 3 cm. a side, of Bradley colored papers. The two pieces of colored paper were directly side by side, in contact, with no intervening space. No principle was used in selecting the components for the color combination. They were divided into two sets of eighteen, one set to be used by each of the two experimenters, and the attempt was made to get into each set some obviously agreeable and some obviously disagreeable combinations. In presenting the cards to the observers, and in instructing the observers, the same procedure was used as in the case of the single colors.

There were in all seventy observers: since each made eighteen judgments, there was a total of 1260 judgments.

The first point to be noted in the results concerns the cases where the observers failed in the effort to change their affective judgments. *A considerably higher percentage of judgments could not be changed when color combinations were used than was the case where single colors were used.* With single colors, the percentage of judgments that could not be altered was 6.3; with color combinations, it was 20.3. A possible interpretation of this result would be that, where the character of the material is the same, the affective attitude is less stable, the simpler its source.

The second point concerns the readiness with which different types of judgment could be changed. Our results *agree with those of the previous study* in showing that *extreme judgments are harder to change voluntarily than moderate judgments.* Of the judgments '1', 29.5% could not be changed; of the judgments '2', 22.4%; of the judgments '3', 11.3%; of the judgments '5', 12.9%; of the judgments '6', 14.7 %; of the judgments '7', 38.5%

Our results *do not*, however, *confirm* the finding of the previous study *that judgments of extreme unpleasantness are harder to change than judgments of extreme pleasantness*; the reverse is true with the color combinations.

Our results *agree* with those of the previous study in showing that *it is easier to lessen the unpleasantness of a color impression by six points than to raise it by six points*. 7.3% of the 1 judgments were raised to 7; 11.4% of the 7 judgments were lowered to 1. We *agree* also in finding that *it is somewhat easier to lessen the pleasantness of a color impression by five points than to raise it by five points*, although the difference here is probably too slight to have significance. 5.3% of the 2 judgments were raised to 7; 8.9% of the 6 judgments were lowered to 1.

Our observers were, like those of the preceding study, asked after a two months' interval to express their judgment of the pleasantness of the color combinations again (no attempt being made to alter them this time).

The first point to be noted here concerns the tendency of the original judgments (made before the effort to change) to recur after the two months' interval. In the case of the single colors, the tendency of judgments of pleasantness to recur had appeared slightly greater than that of judgments of unpleasantness. No such difference appeared in the case of the color combinations.

In the case of the single colors, it was found that "changes *in the direction of increased pleasantness are more likely to be lasting*," where the amount of change is more than one point. We got the *same result with our color combinations*. Of the cases where, in the original experiment, the effort to change the affective value of a color had resulted in a raising of its pleasantness by two points, this increased pleasantness appeared after the two months' interval in 17.2%; of the cases where pleasantness had been lowered two points, 7.6% were permanent; of the cases where pleasantness had been raised three points, 12.4% were permanent; of the cases where pleasantness had been lowered three points, 4.5% were permanent; of the cases where pleasantness had been raised four points, 10.5% were permanent; of the cases where pleasantness had been lowered four points, 10.7% were permanent; of the cases where pleasantness had been raised five points, 9% were permanent; of the cases where pleasantness had been lowered five points, 4.6% were permanent. If these percentages are averaged for the 'raising' and 'lowering' changes respectively, the 'raised' changes show an average of 11.2% of permanence, the 'lowered' changes 6.8%. The corresponding figures from the earlier investigation, with the single colors, were 11.1% and 5.9%.

We may next consider the methods by which the changes in pleasantness of the color combinations were effected, according to the introspective testimony of the observers.

In the case of the single colors, "by far the most frequently used method was the one which we have called 'imaginary context'. The color was thought of in an imaginary setting different from the real one." Special forms of imaginary context consisted in "imaging the color in combination with some other color," and "imagining the amount of the color to be increased." These methods we found to be used in the case of the color combinations, and in addition there appeared the method of imagining a change in the relative amounts of the two colors used in the combination. The method which in the former paper was called 'shift of attention' took on an altered form in these experiments with color combinations. Where the simple impression of a single color was used as the source of the affective reaction, attention could be shifted from a disagreeable to an agreeable aspect of the color, or *vice versa*, only by attending to some association which the color suggested from past experience. Where, on the other hand, the more complex impressions from color combinations were the source of the affective reaction, attention could be, and was, shifted from one color to the other, making the more or less agreeable color dominant in the com-

bination. In treating the results of the present series of experiments, with the color combinations as stimuli, we included cases where the colors suggested associations from past experience under the head of 'altered context', instead of under the head of 'shift of attention'.

The significant results which we have obtained in regard to the methods by which the affective values of the color combinations were changed, as compared with the methods by which the affective values of the single colors were changed, are as follows.

For both combinations and single colors, the occurrence of associated ideas is more likely to raise than to lower pleasantness. In the experiments with the single colors, where changes by this method were reckoned under 'shift of attention', "the average percentage for increasing pleasantness was 24.4; for decreasing pleasantness, 15.3." With the color combinations, where changes due to association were classed under the head of 'altered context', the average percentage for increasing pleasantness was 53.8, and that for decreasing pleasantness 27.1.

There is *less tendency* to alter affective reactions by *imagining the absolute amount (area) of the colors altered*, in the case of color combinations than in the case of single colors. With single colors, 4.2% of the changes were due to this method; with color combinations, 1.4%. It is probably harder to imagine changes in the absolute area of color combinations than in that of single colors.

There is *much less tendency* to alter affective reactions by *imagining another color added*, in the case of color combinations than in that of single colors. With the single colors as stimuli, 17.7% of the changes were due to this cause: with the color combinations, only 3%.

Shift of attention from one color to the other in the combination caused 16.9% of the changes where color combinations were used as stimuli.

Affective adaptation was *somewhat more influential in causing changes in affective reaction where color combinations were used as stimuli* than when single colors were used. With single colors, it was responsible for 2.2% of the changes; with color combinations, for 4.5%. The authors of the study in which single colors were used say: "Probably the slight influence of affective adaptation in these experiments is due to the mildness of the emotional reactions involved."

True compensation, the deliberate assumption of the opposite affective attitude ("I said to myself, 'Now like it!' and I did") *occurs oftener with the color combinations* than it did with the single colors, though it is still rare. It occurred in six tenths of one percent. where the single colors were used, and in 2.5% of the cases where the color combinations were used. The authors of the previous study say: "It is easy to conjecture that this method, so useful in ordinary life, would naturally play little part in conditions where the affective state is not only mild, but accompanied merely by simple motor expressions. One may assume hatred in order to counteract love, for example, because hatred may be expressed by a great variety of movements, by torrents of words, by forcible actions, and when these voluntary movements are set in operation, there is a fair chance that the deeper organic movements associated with them may come into play and the emotion really be transformed. But expressing one's like or dislike of a color is so mild and simple a motor process that its voluntary performance can have no very profound effect." Expressing one's like or dislike of a color combination is also a mild and simple proceeding, yet quite possibly not so mild and simple as that called forth by a single color; and this difference in complexity may be responsible for the greater use of compensation with the color combinations. The number of cases, however, is so small that the difference may be accidental.

REVIEWS OF BOOKS

Human Behavior, in its Relation to the Study of Educational, Social and Ethical Problems. By STEWART PATON. New York, Charles Scribner's Sons, 1921. Pp. 465. Price $6.00.

The need of a study of human behavior more inclusive than has been forthcoming from psychologists has often been felt by those who deal practically with human nature, and especially by the alienists. Definite beginnings of such a study have indeed been made, as, for example, by the psychoanalysts after their manner and by Dr. Adolf Meyer in the "Psychobiology" which he has been teaching for a number of years at the Phipps Clinic. Dr. Paton's book is a contribution from the point of view of the biologist and the physician to this same admirable and difficult undertaking. Its main thesis is that adult human behavior can be properly understood only when studied in all the light that can be shed upon it by the comparative study of behavior in less developed organisms—both those lower in the animal series and earlier in the human series, including even the embryo—and of the disordered behavior of the insane. The behaving individual must also be studied both as a unit, neither mind alone nor body alone, and in its dependence upon all its several systems—not alone upon the central nervous system, but upon the autonomic, the endocrine, the circulatory, the muscular, and the vegetative.

In supporting this thesis in detail Dr. Paton discusses the adjusting mechanisms (nervous and non-nervous), personality and its development, temperament, character and intelligence, the mechanisms of control, dispositions, habit formation, the involutionary processes of old age and the conflicts and dissociations of imperfectly organized personalities. Other chapters treat of the historical development of this general point of view, of the methods to be used in the study of personality, of education and of the broader benefits which may be hoped from a perfected science of human behavior. In its major outlines the book is excellent. Its standpoint is sound and the author's insistence upon it is timely; his account of present day knowledge of the biology of behavior is complete and authoritative; he points out many promising openings for research; his incisive criticism of current educational practice is deserved.

But the psychological reader, even though he be a sympathetic one, is apt, nevertheless, to lay the book down with something of disappointment. First because it demonstrates all too clearly that the study of human behavior, though it draws upon established sciences, has not itself as yet fully reached the scientific level. This appears not so much in the incompleteness of present information, which Dr. Paton notes at many points, as in what he himself says incidentally with reference to terminology and methods. On p. 50 he urges the retention of the word "consciousness" in spite of its manifold and recognized ambiguities. On p. 355 he notes that "as soon as we abandon the attempt to seek for rigid definitions we shall—as has often before been the case in the development of the study of human activities—find ourselves in a far better position to interpret the significance of processes which are now only partly understood." And again on p. 372 he warns the would-be student that the successful examiner of personality is "born not made" and that "one may acquire, by diligent practice and long experience, some considerable skill in exploring and exposing the basic influences which condition character, but there is a point in the analysis beyond which the examiner cannot go unless he himself possesses the peculiar adaptiveness and insight associated with a natural

aptitude for making personality studies,"—all of which is probably true, but could not be said truly of a study already upon its feet as a science. Perhaps, indeed, no science of human behavior is possible.

For this cause of disappointment the psychologist may well blame his own over-sanguine expectations, but for two others he may fairly hold the author to account. In the first place the work bears the marks of haste in preparation or at least of having missed a final revision. It is uneven in style; a paragraph here and there reads as if it had been incorporated without change from the author's notes of his reading and in consequence is difficult to understand without as full a knowledge of the original; others are clumsily worded and obscure without even this excuse. Superficial contradictions, too, occur here and there. We read, for example, on p. 81 that "there seems no evidence that warrants the attribution of any specific form of psychic activity to the cerebral cortex," but on p. 63 of "the higher complex faculties described as reason—a function of the cerebral cortex," and on p. 144 that "the higher cortical centers of the brain are those in which the mechanisms of self-consciousness are chiefly represented." On p. 147 we hear of "the vague self-consciousness which dawns at birth," but learn on p. 154 that "gradually . . . . as the muscular system falls more and more under the control of the brain and both the sensing activities and their coördinations become greater, there emerges the complex of responses which we recognize as the first indication of the dawn of self-consciousness." These are rather trivial matters perhaps, but obscurity and carelessness help neither the reader nor the cause which Dr. Paton has at heart.

That Dr. Paton's psychological terminology would have been reformed in a last blue-penciling is by no means sure, but it is a pity that he could not have had at the critical moment the aid and comfort of some competent colleague. He would have learned that no psychologist writes of "sensing . . . . ideas" (p. 118), or of a "sense of appreciation dependent upon the evaluation of muscular contractions" (p. 141) or of "a sense of credulity" (p. 327), and that no psychologist, except in joke, would give his endorsement to such a statement as that which is cited on p. 279, to wit, that "one-third of our mental make-up is instinct, one-third habit and one-third a process of becoming one or the other." Among the nearly 400 authors to whom Dr. Paton refers there are a goodly number of psychologists, but he has evidently proved immune to their manner of speech, if not also to their manner of thought.

When a science of human behavior comes, if it ever does, it will come as the joint work of the biologists and psychiatrists on one side and of psychologists and sociologists on the other, and each party will have to know well and give respectful attention to the work of the other. Dr. Paton presents us with an excellent account of the data which his side can now contribute. We trust that his acceptance of a place on the programme of the recent meeting of the American Psychological Association at Princeton argues an inclination on his part toward a better acquaintance with working psychologists and a more intimate knowledge of their science.

E. C. S.

Psychology: A Study of Mental Life. By ROBERT S. WOODWORTH. New York. Henry Holt & Co., 1921. Pp. x, 580.

In an easy and conversational style, Professor Woodworth presents in this book his idea of modern psychology, which, as he says in his opening sentence, "is an attempt to bring the methods of scientific investigation........to bear upon mental life and its problems." Abandoning the traditional isolation and independence of psychology as a separate science, he adopts the point of view that it is a department or branch of biology. He regards psychology as "the science of the conscious and

near-conscious activities of living individuals" (p. 17). The prototype of the conscious and near-conscious activities is the simple reaction, which is "a *response* to a *stimulus*" (p. 22). This schema of stimulus-response is of great systematic importance. It appears in the exposition of all the activities, the conscious and the near-conscious, the higher and the lower, the native and the acquired, and it forms the scaffolding of the entire work.

In the opening chapter, Woodworth describes the subject-matter, problem, and methods of psychology.

The subject-matter of psychology is the mental activities, which form a sub-class of the vital activities. The activities considered are those of individuals as distinct from those of groups and from those of single organs: a distinction which separates psychology from sociology on the one hand and from physiology on the other. The limiting adjective 'mental' is merely relative, for the near-conscious activities—those closely related to consciousness—belong properly to the subject-matter of psychology. Within these broad limitations the activities which psychology studies may be human or animal, adult or child, normal or abnormal.

The problem of psychology is threefold: to determine how individuals differ in their mental activities, how they resemble one another in their mental activities, and how the study of these differences and these likenesses may be put to practical ends.

The general methods of psychology are the experimental, comparative, genetic, and pathological. With every one of these two specific modes of approach may be followed: the introspective (the mode peculiar to the science of consciousness), and the objective (the mode peculiar to the science of behavior). Woodworth employs both.

With the field thus defined, "the next question is where to commence operations" (p. 21). The answer, of course, is "to start with the simplest sorts of mental performance, either with sensations, as do the introspective psychologists who think of sensations as the chief elements of which consciousness is composed, or with reflexes, as do the behaviorists who conceive of behavior as composed of these simple motor reactions" (p. 21). The cue is taken from the behaviorists "because the facts of motor reaction are more widely useful in our further studies than the facts of sensation, and because the facts of sensation fit better into the general scheme of reactions than the facts of reaction fit into any general scheme based on sensation" (p. 22). A further "advantage of basing our psychology on *reactions* is that it keeps us 'close to the ground', and prevents our discussions from sailing off into the clouds of picturesque but fanciful interpretation. Psychology is very apt to degenerate into a game of blowing bubbles, unless we pin ourselves down to hard-headed ways of thinking. The notion of a reaction is of great value here, just because it is so hard-headed and concrete" (p. 68).

The reaction is therefore accepted as the fundamental basis of psychology, and is defined as "a *response* to a *stimulus*." A stimulus is any force or agent that, acting upon the individual, releases energy stored in the organism; and a response in the simplest case is a muscular movement, in more complex cases any act of the individual organism: a feeling, sensing, doing, observing, knowing, inferring, etc.

Working from the simple to the complex, Woodworth in Chapter II considers the reflexes and other elementary forms of reaction, and then advances in Chapter III to the reactions of the higher levels. He argues that under his definition "there is no reason why we should not include a great variety of mental processes [sensations, perceptions, thoughts] under the general head of reaction" (p. 45).

In Chapter IV purposes, motives, interests, or tendencies (as they are more aptly called), which at first thought seem to transcend the 'stimulus-response' point of view, are reconciled to it. Woodworth finds that a pur-

pose is itself an inner response to some external stimulus, which persists for a time, and which acts in its turn as a central stimulus to further reactions.

With this "stock of methods and general concepts," Woodworth turns in Chapter V to the consideration of the case of democratic John Doe: which of his traits are native and which acquired? Proceeding upon the criterion of universality, that that trait which all individuals of the same descent show in common is native—unless evidence can be brought forward to the contrary (p. 98), Woodworth classifies the native traits as: reflex, instinct, emotion, feeling, sensation, attention, and intelligence. These traits he describes, so far as possible, from both the objective and the subjective points of view.

Reflex and instinct are described only from the objective side. The reflex is a motor or glandular response; the instinct is a motor or glandular tendency. Neither is conscious, but both are near-conscious in that they are somehow like the conscious.

Emotion is described from both points of view. On the objective side the emotion, which (as the inventory of the instincts and emotions in Chapter VIII shows) is closely bound up with instinct, is a neural response. It is the end-result of a number of internal and external preparatory acts of muscles, organs, and glands. On the subjective side it is closely bound up with impulse, with the conscious tendency of "wanting to do something." Considered apart from this tendency, however, emotion is a "conscious stir-up," a "feeling somehow," a "mass of sensations" aroused by objective bodily changes.

"Feeling is subjective and unanalyzed"; "it is simply the 'way you feel'" (p. 172). While conscious, it is not cognitive; for "as soon as you begin cognizing and say, 'I feel badly here or there, in this way or in that,' you know something about your subjective condition, but the feeling has evaporated for the instant." On the objective side "feeling is an impulse to 'stand pat' or to end the state;" it is, in other words, a specific neural tendency.

Sensation on the subjective side is bare conscious response. "The child does not learn to see or hear, though he learns the meaning of what he sees and hears. He gets sensation as soon as his senses are stimulated, but recognition of objects and facts comes with experience. Hold an orange before his open eyes and he sees, but the first time he doesn't see an orange. The adult sees an object, where the baby gets only sensation. 'Pure sensation,' free from all recognition, can scarcely occur except in the very young baby" (p. 187). On the objective side "sensation may be called the first response of the brain to the external stimulus." "Without the brain response, there is apparently no conscious sensation, so that the activity of the sense organ and sensory nerve is preliminary to the sensation proper." After describing the various sense organs and discussing the elementary sensations and their blends or compounds,—a discussion marked only by unqualified acceptance of the Ladd-Franklin theory of vision,—Woodworth turns to the consideration of adaptation, after-image, and contrast. This chapter on sensation, he tells us in the preface, "might perfectly well be omitted......without appreciably disturbing the continuity of the rest."

"Attention is preparatory, selective, mobile, highly conscious." "To attend to a thing is to be keenly conscious of that thing, it is to respond to that thing and disregard other things, and it is to expect something more from that thing" (p. 244). Attention is a complex response. "Its natural stimulus is anything novel or sudden, its 'emotional state' is curiosity or expectancy, and its instinctive reaction consists of exploratory movements, its inherent impulse is to explore, examine, or await."

On the subjective side attention is highly conscious. "One of the surest of all introspective observations is that we are more conscious of

that to which we are attending than of anything else." On the objective side attention is a neural and motor reaction: neural, in so far as it is a mental activity; for degree of consciousness tallies, not with intensity of sensation or energy of muscular action, but with degree of mental activity; and motor, in so far as the movements that occur in attending to an object are such as to bring the sense-organs to bear on that object as efficiently as possible.

Intelligence comprises the organism's innate limitations and susceptibilities of reaction. On the subjective side intelligence is like reflex and instinct, *i. e.*, it is near-conscious. On the objective side it resembles instinct in that it is a native tendency; but instinct consists in ready-made native reaction-tendencies, whereas the intelligence of an individual at any age depends on what he has previously learned. It might appear from this statement that intelligence is not a native but an acquired trait; but, in fact, what the individual learns depends upon his retentivity, his responsiveness to relationships, his persistence, his submissiveness, his curiosity, and his special aptitudes.

The acquired reactions and tendencies to react—all knowledge, the whole stock of ideas and of motor skill, and certain motives and likes and dislikes—are modifications of the native forms just mentioned. In the next four chapters the processes of learning and acquiring these modifications are described. The first is devoted to the acquiring of motor habits and skill; the second, to memory; the third, to acquired mental reactions; and the fourth is given to association, to a consideration of the general laws of exercise and of combination which hold sway in the whole field of acquisition of reactions.

At this point the decision is reached that John Doe's "behavior is primarily instinctive or native, but that new attachments of stimulus and response, and new combinations of responses, acquired in the process of learning, have furnished him with such an assortment of habits of all sorts that we can scarcely identify any longer the native reactions out of which his whole behavior is built." Henceforth we must keep him under surveillance in order to see what use he makes of this vast stock of native and acquired reactions, how he behaves from day to day, and how he meets the exigencies of life (p. 419). Study reveals that "his life is a voyage of discovery and at the same time a career of invention" (p. 421).

The culmination of the process of discovery is perception. On the subjective side "it consists in responding to a stimulus by knowing some fact indicated by it either directly or indirectly" (p. 422). Sensation gives us the sign of some fact; perception, the meaning of the sign. On the objective side perception is a secondary response to a physical stimulus, being properly a direct response to the sensation. "The chain of events is: Stimulus, response of the sense organ and sensory nerve, first cortical response which is sensation, second cortical response which is perception" (p. 423). It is only, however, the simplest perception which is thus singly determined; ordinarily it takes a collection of stimuli to arouse a perception. This collection is at the same time a selection (under the laws of attention and association) from the whole mass of sensory stimuli acting at any moment on the individual. "Perception is at once an isolating and combining response" (p. 431). Reason is the counterpart of perception; "indeed in discussing reason we are still on the topic of perception." The reasoner is an explorer, and the goal of his exploration is the perception of some fact previously unknown to him. Two facts are present as stimuli, and the response, which on the subjective side is called inference, consists in perceiving a third fact that is implied in the two stimulus facts. "Inference, typically, is a response to two facts, and the response consists in perceiving a third fact that is bound up in the other two." On the objective side reasoning is the same as perception, a neural response of the cortical regions adjacent to the sensory areas.

From exploration and discovery, which are involved in perception and reason, we turn to manipulation and invention, which are involved in imagination. The stimulus consists of facts, either perceived at the moment or recalled from past perception, that are now freshly related or combined. The response may be divided into two phases: preliminary, receiving a combination of stimuli; and final, responding to the combination. Typically, the preliminary stage consists in the recall of facts previously perceived. The final stage is invention, which consists in a response to the novel combination of facts.

Will and Personality, the subjects of the closing chapters, are not psychological terms in so far as they are not special kinds of responses. Will refers to certain relationships in which a response may stand to other responses; personality, to the array of native and acquired traits. Under the heading 'Will' Woodworth discusses the various kinds of action and the practical applications of the subject, "how to get action either from yourself or from others." Under 'Personality' he discusses the factors involved, the development and expansion of the self, and the unconscious mind. In his consideration of the unconscious, he severely criticises the concepts and tenets of Freud and of the psychoanalytical school.

We have here an empirical biological system which has been cast in the mould of "stimulus-response," a mould which even cursory study reveals as inadequate. The concept is teleological throughout, and the logical consequences of interactionism can not be escaped.

Few definitions are attempted. Woodworth is satisfied for the most part to rely upon the common-sense appraisement of his terms. Where this seems inadequate, and he does attempt definition, the formulas are for the most part vague and uncertain. For example, the definition of reaction, the unique and fundamental concept of the system, is "a *response* to a *stimulus*." Yet nowhere do we find an exact definition of either of these terms. A stimulus is "any force or agent that acting upon the individual arouses a response;" but what, then, is a force or agent? is it physical or mental or physiological? and how does the undefined force or agent act, and what does it act upon? Woodworth replies that it acts upon the individual, but again he nowhere tells us what he means by an individual. What does the force do to this enigmatical individual? We are told that it "arouses a response." And response is described as any act of the individual organism aroused by a stimulus. Tautology, pure and simple! One member is defined in terms of the other. When we turn to the acts themselves, we find that acts of the muscles, glands, organs, end-organs, nerves, sensory centers, cortex, as well as stir-ups, wanting-to-do-something, feeling, sensing, highly conscious consciousnesses, observing, doing, knowing, inferring, inventing and modifying appear on the scene. But these activities belong to very different universes, and can be brought together under the concept of reaction only because of the looseness of the terms employed.

Psychology does not, however, include all the activities that can be grouped under the concept of stimulus-response, but only those that are "conscious or near-conscious," only those that come from "the organism or individual as a whole." Upon these principles, Woodworth excludes the activities of digestion, of the circulation, of the liver, etc. The limitation of subject-matter would be more comprehensible if "conscious" and "near-conscious" and "organism as a whole" were unequivocally defined. They are not; and we find that the author's exposition but adds to our perplexity. On p. 73 he illustrates the organic level of purposive behavior by reference to the stimulation of a single muscle, which by his own criteria must be physiological! Again it is difficult to see, on the basis of relation to consciousness, why he includes reflex, instinct and intelligence and excludes digestion, circulation and breathing. On the basis of

extent of activity, whether of a single organ or of the whole individual, the case is even more puzzling. The activity of the circulatory system, which extends throughout the entire body, is excluded, whereas the isolated pupillary reflex is included. It is also not easy to see why Woodworth includes emotion and attention among native traits. Emotion, he says, "consists wholly of the sensations of bodily changes." Why, then, is it listed as a separate trait? Why is it not included in the chapter on sensation and discussed as a blend? Elsewhere, we read that "all the emotions belong under the general heading of the feelings." Attention from the objective point of view gives us nothing new; it practically amounts to emotion, instinct and impulse. From the subjective point of view, however, we find that "we are more conscious of that to which we are attending than of anything else;" attention is a high degree of consciousness. Since degree of consciousness tallies, not with intensity of sensation or energy of muscular action, but with degree of mental activity (p. 266); and since mental activity is defined as conscious or near-conscious activity; it follows that attention is a high degree of consciousness which goes with a high degree of conscious or near-conscious activity.

The discussion of attention brings up the question of degree of consciousness. What does this expression mean? How can there be, for instance, degrees of consciousness in the field of sensation? This trait, be it remembered, is not cognitive. When we pass to the more complex levels, awareness and cognition appear, and degrees of consciousness find ready application. Still, even at the cognitive level, the concept is not without its pitfalls; it implies a permanent mind, a knower, and carries us back to the old 'self-activity' psychology. This implication is also present in Woodworth's concept of tendency. His exposition is straightforward and clear in the explanation of tendencies and the treatment of native traits; but in his discussion of the acquired traits and of the modifications which follow the laws of exercise and combination the implication of a permanent mind is very apparent. For example: how can use or exercise modify a native trait? Modification is explainable only by the tacit assumption of the middle term, the tendency, which is effective in both directions: toward the stimulus, in the substitution and detachment of the original stimulus; toward the response, in the substitution and combination of responses.

The style of the book is colloquial, facetious, and even slangy, as some of the excerpts already quoted show, and as the following remarks will further illustrate: "The dog passed another on the way without so much as saying 'How d'ye do?'"; "We had better fetch that law out again and put it in good repair and see whether it is adequate for the job that we now have on hand;" "Now bring in our trusty law of exercise;" "The law of combination seems to fill the bill very well;" "Errors of any kind are meat to the psychologists;" "Not that Freud would get our OK." It is at least doubtful whether this innovation is of advantage to a scientific text-book.

K. M. D.

Die Grundlagen der psychischen Entwicklung. Eine Einführung in die Kinderpsychologie. By K. KOFFKA. Osterwieck am Harz. 1921. Pp. viii+278.

This book is more than a restatement of facts and inferences regarding mental development and the psychology of childhood; it is a reinterpretation of the genesis of mind according to certain principles of mental structure which have grown out of the recent experimental work on *perception* undertaken by Köhler, Wertheimer, Koffka, and their collaborators. The new point of view is radical in its abandonment of the conscious element as a unit of structure, for with this go also the law of association,

as commonly understood, and likewise the well-worn mechanistic conception of interconnecting neural pathways. In place of the older theory of mind as originally a chaos of discrete elements which must be associated together by frequent repetition in order that certain 'bonds' of connection may be established as habits, the new structural psychology finds a 'closed system' already formulated as an original phenomenal datum. Corresponding to this is a 'closed system' on the physical side which replaces the discrete stimulus; while mediating between the two are the functional uniformities of the organism in its biologically adaptive processes.

From this point of view the mechanistic interpretations of behavior advanced by Thorndike and Watson come in for detailed criticism. Over against the experiments of the American students of animal behavior are placed those recently carried out by Wolfgang Köhler at the experimental station established by the Prussian Academy of Science at Teneriffe. As an indication of the difference, both in method and results, which arises from so radical a difference in the point of view from which the problem of behavior is attacked, we may state the case briefly as Koffka presents it.

Thorndike, in his famous puzzle-box experiments, confronts the animal with a task which can be solved only by chance. The appropriate response by which release from the box is obtained is not one which the animal could readily understand, even if it had the capacity of understanding. Hence the trial-and-error method employed is both tedious and wasteful. That the animal does, nevertheless, succeed eventually in mastering the situation is not explained by repetition alone, as Thorndike admits; nor is it explained by the satisfaction that attends an effective performance, since neither a preformed neural readiness to do the act in question nor a pleasurable nuance attaching to this fortuitous activity can be assumed. The animal learns here, as elsewhere, according to Koffka, by the formation of a functional structure which includes in its continuous course the 'situation' of hunger and irritability upon confinement, the selective manipulatory act of release, and the subsequent enjoyment of freedom and food. But the conditions under which this complex series of states and events is formed are the worst possible in which to reveal inventiveness or intelligence. Hence the act is but slowly acquired, and readily forgotten, because the important factors in the total structure are motor rhythms which do not naturally find a place in the original impulsive situation. Yet even here the type of activity involved in the release has an important bearing upon the efficiency of the animal's learning. When Thorndike taught his cat to lick itself before he would open the cage and release it, he found that the act in question rapidly degenerated into the merest rudimentary gesture; and if not then given its freedom the cat failed to repeat the performance immediately as it would always do when its act had a direct bearing upon the mode of release. Thorndike was unable to explain this fact, but Köhler's observations lead him to conclude that the behavior of the animal is always typically different when the act it is called upon to perform is, from its point of view, senseless. An efficient performance in solving a problem always involves some 'insight' on the part of the animal, that is, the means taken to achieve the end always indicate an inner structural connection.

Thus Köhler in his experiments with chimpanzees and hens always planned a situation where insight was possible, leaving the results to determine whether it was present or not. In general the arrangements in Köhler's tests were such that, in order to secure a reward, the animal must discover an indirect means by the use of a tool or by overcoming some hindrance in its way. Thus a basket of fruit placed outside of the ape's [illegible]h could be secured by pulling on a string attached to the basket, by [illegible]g it forward with a stick, or by mounting upon a box which the ape [illegible]irst place under it. These tasks were not always easy to perform, but

the achievement, after many vain attempts interspersed with periods of rest, was always a sudden and immediate reaction in which the means employed in attaining the end had all the characteristics of an intelligent discovery. One of the most interesting of these discoveries was the use made of two pieces of bamboo by putting the smaller into the end of the larger to give a needed extension. The ape had previously poked out one stick by means of the other until it touched the basket, but it was only in what appeared to be an idle moment of play that the discovery of fitting the smaller into the larger stick was made. At once the animal went for the basket with the now lengthened stick, and though the sticks fell apart because they had not been well put together, the ape was not deterred in its purpose, but refitted them again, and fetched in the fruit.

That structural formulations are original phenomena of the mind, and not a product of previous learning, is indicated by the experiments with hens, a species not precisely noted for intelligence. Having found that hens can discriminate light and dark papers, these were placed on food boxes and the hen trained to select the box with the darker label. In the course of the experiment care was taken to alter the position and also the papers, so that the cue should be given by the specific brightness and not by any other factors. After training, 'critical' tests were made in which a lighter or a darker color than either of those in the training series was exposed along with the 'positive' color which the hen had been taught to select, or the 'negative' color which it had been taught to reject. If behavior is originally determined by a discrete stimulus we should expect the hen always to select the positive color, whenever present, and likewise always to reject the negative color. But this was not the case; if the hen had been taught to select the darker of two colors, it continued to select the darker, even when the box in question had been the 'negative' box in the training series. Likewise it rejected what had been the 'positive' box in favor of one whose color label was still darker.

From such results as these Koffka elaborates a theory of structural phenomena upon the basis of which he proceeds to give a reinterpretation of mental and bodily development, the nature of reflexive and instinctive activity, and the mode of learning. The genesis of a phenomenal structure is the appearance of a *quality* limited and somewhat definite on a background which is both vague and indefinite. These are the most primitive phenomena of perception in which every member of the structure carries all the other members along with it. Thus, the discovery of any particular member, as such, is the creation of a new structure, which has not previously existed. Whatever arises in consciousness as a structure is something to which the organism reacts, being conditioned not only by the corresponding structures involved in the physical impression, but likewise by biological needs and impulses of behavior. Thus 'friendliness' and 'unfriendliness' are more primitive structures than a *blue spot*, and in the original structures the affective elements are in no wise distinct from the perceptual. The assumption that a sensation is determined for all time by its stimulus must be given over, for a sensation is only a highly elaborate and refined structure, entirely a product of analysis, and in no sense an original datum of mind. Thus, for example, the color discrimination of infants, to which the author gives detailed consideration, must be reinterpreted in terms of structures, light and dark, 'warm' and 'cold', colored and uncolored, in place of the discrete sensory elements which have hitherto been assumed to indicate the presence or absence of color sensitivity.

Transfer of training is readily understood as an acquisition of structures; for within a broader structure a partial structure of similar form may find facilitation of expression, whereas Thorndike's doctrine of 'identical elements' makes no such provision for flexibility of application. Again, learning to read by sight through sheer repetition is futile, because one never learns anything unless opportunity is given for recitation in

which the material can be worked over into a structural form. Learning is never a mere connection of impressions or acts, but always a certain *way* of acting. But, a certain way of acting having been learned, quite different materials both of perception and of response may be suited to the performance; for it is not the materials, either as sensory ingredients or as specific muscular reactions, which constitute the phenomenal structure that has been learned, but something quite different, conditioned by the functional uniformity of the organism in its biological processes.

Here, however, the author leaves us in doubt, for apparently he has no psychological doctrine of integration by means of which the analytic data of the phenomena of perception are shown to constitute these mental structures. If the same structure may arise under totally different conditions of stimulation and persist through various muscular adjustments of response, is there no strictly phenomenal pattern by which the identity of the structure can be gauged? It would seem that Koffka regards this identity of structure as sufficiently defined in terms of its biological function; but though he attempts to overcome the opposition of mechanism and vitalism as alternate explanations, by an interpretation which is neither the one nor the other, he is not altogether successful in making clear his emancipation from teleology. Much attention is given to the variety of structures, primitive and acquired, and also to the genesis of series and groups in perception and in movement, but one is troubled by the lack of an unequivocal determination within the phenomenal realm of consciousness itself.

In considering the tests of Köhler on apes and hens with respect to the apparent size of objects and the surface-color of labels, one might suppose from the results obtained that within the structural range of an ape's view there is no phenomenal difference in the perceived size of an object as it recedes into distance, and likewise that the surface-color of a neutral grey remains phenomenally constant even when its comparative brightness has been reversed with reference to the 'negative' color of the training series. The facts as indicated in Köhler's careful tests are unimpeachable, yet no attempt seems to be made to account for them in phenomenal terms. A similar criticism has been made of Wertheimer's 'phi phenomenon,' as the essential structure of visual movement; but Dimmick found conscious data for the 'phi phenomenon' as a grey flash integrating with an attribute of duration.[1]

Even if we are now led to relinquish elemental sensations and their association as the groundwork of psychology, must we turn to biology, with its teleological or mechanistic implications, for the interpretation of every mental structure that a concrete act of behavior may imply? Is it not rather the problem of psychology to investigate these mental structures as integrations of qualities involved in their membership, in order that the gross nature of the structured *quale* may be reduced to attributive aspects such as qualitative modality, intensity, duration, spread, etc., each one of which can be controlled by a corresponding variation of the stimulus? One has an uneasy feeling that these new phenomenologists of mental structure are too ready to posit the phenomenal side of behavior as unique data which cannot be more closely defined. Yet a structure whose members are incapable of definition is hardly a scientific datum, and if every definition must be functional, that is, organic, psychology is still thrown back upon the tender mercies of biology, just as surely as the behaviorist argues that it must be. Even though the physical universe itself, which supplies the 'situations' for behavior, is rightly conceived as an elaborate interconnection of structures to which no mechanistic interpretation of accretion and summation can ever do justice, yet the uniformities of the physical universe do submit to definition. Why not also the corresponding uniformities of the mental structure, and, indeed, in much the same way?

[1] Cf. this JOURNAL, xxxi., 1920, p. 317 ff.

These are but some of the questions which come to one's mind after attempting to digest this somewhat bewildering review of the subtleties of mental development and child-life that Koffka has managed to compress within the limits of a small book. We have neglected to trace the outline of his treatise, and have omitted many illuminating inferences which carry us far beyond the crude and stodgy interpretations which are still current in present-day American pedagogy. But it seemed more important, in attempting to bring this very significant point of view to the attention of American readers, to devote our space to the new theory and certain of its implications, rather than to review the book with reference to its specific content. We may venture to hope that the volume will be widely read by educational psychologists, because it is filled with nuts for the behaviorist to crack, and if not all are ready to throw over their pet theories of applied psychology in favor of the one here proposed, no one can fail to benefit from the redefinition of his conceptions which the reading of this book will force upon him.

R. M. Ogden

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A Treatise on Probability. By John Maynard Keynes. London, 1921. Pp. xi + 466. Price 18s.

The author of the *Economic Consequences of the Peace* here sets forth a logic of probability, an attempt to treat in comprehensive and systematic fashion the problems of the philosophy of induction with the related general principles of mathematical probability and statistical inference. The work belongs to the course of thought initiated by Leibniz, left undeveloped for a time, brought again into prominence by Hume and Mill, and revivified and applied to the concrete by Laplace and Jevons and Venn. The treatise itself was begun as a fellowship dissertation at Cambridge under the influence of W. E. Johnson, G. E. Moore, and Bertrand Russell, to whom it owes much both for problem and method. After the interruption due to war work, it is now modestly presented "for criticism and enlargement at the hands of others." It intends to supplant by constructive theory some of the negative criticism contained in the chapters on induction in our present-day texts on logic, but it offers also a wealth of critical material to mathematicians who are willing to be concerned with the presuppositions of the theory of probability, as well as a variety of suggestion and caution to all who make use of statistical methods in seeking scientific conclusions.

The book is divided into five parts, with an index, and a selected and briefly annotated bibliography of 25 pages.

Part I gives the general epistemological setting. Probability is defined in its widest sense as a *relation* between propositions (premises) which are derived by direct knowledge, and other propositions (conclusions) which are derived from these indirectly by argument (inference). If not completely implied in and deducible from the data (premises) themselves, all conclusions thus have only a certain probability, and all true induction is effected by the application of a general theory of probability.

Probability, being itself a relation, is always relative to the data upon which it is based. A given conclusion may be probable on one set of data, and improbable on another set. Independent certainty, therefore, can be regarded only as the limit of all possible probability relations, based upon infinite (and therefore practically unattainable) data.

Keynes holds that probabilities, thus generally defined, are not always comparable, even theoretically. Expressed metaphorically, the "path" from data to conclusion is not always straight, and probabilities lying on two different paths may belong to different and incomparable orders. The measurement and mathematical comparison of two probabilities are

possible only when both lie in the same path. This occurs only when the conclusion is one of several exhaustive, exclusive, and equiprobable alternatives. Theoretically such a conclusion exists only for the problems of predicting tosses of a coin, balls drawn from a bag, and the like. The law of non-sufficient reason and the "frequency" theory of probability based upon it are applicable in such cases, but their application to apparently similar practical problems can be made only with reservation and great circumspection. In thus extending their application, the determination of the relevance of data to conclusions is critical. Nevertheless, at least in concrete practical problems where conditions are complex, our judgments of relevance must depend not upon rules, but upon direct insight.

Common uncritical acceptance of the frequency theory has tended to obscure the distinction between the probability and the "weight" of an argument, although it is clear that added evidence, while always increasing weight, may increase, leave unchanged, or actually decrease the probability of a conclusion. The common reliance upon the "probable error," as a measure of weight, is therefore justified only to the extent that further data are more likely to include more relevant and significant data. The probable error, as a partial description of observed facts, must not be confused with its use as an indication of the precision of a generalization regarding all (both observed and not observed) facts of the kind in question.

Part II restates the fundamental concepts and definitions of Part I in terms of symbolic logic. Thus, a/h represents the probability of conclusion a derived from premises h; $a/h=1$ represents certainty; $a/h=0$ represents impossibility, etc. From such simple definitions and axioms, the author develops the "laws of thought" or "necessary inference" (contradiction, excluded middle, etc.) and the theorems of "probable inference" (addition and multiplication of probabilities, inverse probability, etc.)

The remainder of Part II deals with the application of these theorems to various problems. First comes a treatment of the theoretical probability of testimony and the credibility of witnesses. Psychologists and especially those concerned with statistics and with theory of psychophysical method will find a wealth of new and particularly interesting material in Keynes' discussion of the assumptions ("laws of error") which are logically implied in our determinations of the most probable value of variable measurements by the method of least squares, and in our common use of the various averages, the median, etc., as representative values.

Part III contains a discussion of the assumptions which underlie induction, and a logical analysis of inductive generalization. Induction, in its most general form, consists in the observation of a limited number of things, all of which are found to have certain common and therefore essential properties (a known positive analogy), but some of which also are found to have, and others to lack, certain irrelevant properties (a known negative analogy). If we assume that the properties of natural things actually occur in finite "bundles", or are the result of a finite number of "generators," then knowledge of the essential nature of a thing can be increased by seeking and finding additional cases in which new and inessential properties appear. Induction, that is to say, proceeds by increasing the negative analogy, and thus more and more closely limiting the actually essential properties to those of the known positive analogy. Mere multiplication of instances as such would be logically valueless except for the *probability* that the new instances will, as a matter of fact, differ from those already examined in points of unknown but actual positive analogy.

Part IV begins with a discussion of "objective chance" and "randomness." The usual definitions imply the frequency-theory of probability. The author, therefore, redefines the concepts in terms of the general theory previously outlined.

Objective chance does not mean lack of determination,nor does it necessarily indicate a small probability of the occurrence of any given outcome. It exists when we suppose that no increase in our knowledge of the laws of nature and no *practicable* increase in our knowledge of conditions and principles or relevant causal laws will enable us to form more probable conclusions. Random selection exists when, in choosing a member from a group or class, we have no *a priori* warrant for supposing that we shall obtain any one particular member rather than any other, or when such knowledge as we have regarding the particular member obtained is irrelevant to the question whether or not this member actually possesses the characteristic under examination.

The definitions thus arrived at are then applied to a variety of problems: to the question of randomness in the distribution of angular inclinations of the orbits of planets, of random causes for the existence of binary stars, and in the existence of star drifts; to the questions of final causes and argument from design and to the problems of psychical research, etc. Finally comes a discussion of the application of probability to conduct. Here such questions as those of "moral risk" and the "Petersburg paradox" are treated in considerable detail.

Part V is a critical study of the foundations of statistical inference. The first chapter brings out briefly but emphatically the distinction between the purely *descriptive* use of statistics as applied to observed instances, and the far more difficult and uncertain use of statistical methods as a basis of inductive *generalization*. The following chapter gives briefly the historical setting of modern statistics, and deals chiefly with the "law of great numbers" as developed by the theoretical work of Bernoulli and Poisson and supported by the investigations of Quetelet. A great deal of later unjustified application of these principles and theorems, and of the modern preoccupation with the mathematics rather than with the logic of statistics, is to be attributed to the insufficiently critical attitude, the too general claims, and the sometimes unguarded language of these earlier statisticians.

Keynes then considers the conditions which must exist if Bernoulli's and similar theorems are to hold for various theoretical and practical cases. Bernoulli's theorem, for example, is rigorously applicable only if a knowledge of what occurs in early observations will not affect the probability of what will occur in later trials; and, secondly, if these probabilities are all equal among themselves. It can be applied for prediction in 1000 tosses of a coin only if our initial assurance as to the trueness of the coin and the conditions of tossing is so great that 999 "heads" in succession would not alter our half expectation of "tails" on the 1000th toss. "It is, in fact, difficult to give a concrete instance of a case in which the conditions for its application are completely fulfilled." This point is exemplified by an account of the attempts to verify the theorem *a posteriori* by actual coin tossing, dice throwing, lotteries, and roulette. Poisson's theorem accepts the second assumption but not the first, and the still further generalised forms of the theory due to Pearson, Czuber, Tchebycheff, and Simmons, though useful for particular instances, get us really little further in the general treatment of concrete cases.

The author next discusses the problem of the determination of general probabilities from numerical frequencies actually discovered for a group of instances of apparently similar character. He here severely criticizes the lack of logic involved in Laplace's law of succession and shows the absurdities to which its application may lead. The inversion of Bernoulli's theorem is held to be rigorously applicable only if we know that our original data are a true random sample. This knowledge, as already pointed out, is in most concrete cases practically impossible. [Psychologists who find a difference between the means of two *apparently* (but not *certainly*) unselected groups ought not to be surprised if on the next trial with two

apparently similar groups the direction of difference is reversed.] Numbers and mathematical treatment as such can never lead us to truth, apart from consideration of the methods and conditions and more general inductive procedures which we use in obtaining the numerical results. The beginnings of a truly critical work in this connection have been made by Lexis and von Bortkiewicz, a brief account of whose methods concludes the treatise.

It is easy to say in conclusion that logician, mathematician, and man of science owe a debt of gratitude to the author of the treatise for making a first truly comprehensive and critical study of questions which are of fundamental significance to all three disciplines. Not all logicians, of course, will agree that the distinction between knowledge gained by "direct" perception and that gained by argument is logically fundamental. Before we can apply the author's principles of induction to psychical research, for instance, we must have a theory of perception which will enable us to say more definitely whether or not we can directly perceive "spirits", a question which the author's system leaves open. Detailed and critical evaluation of the treatise, however, must be left to those more competent than the present reviewer.

William S. Foster

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Education and World Citizenship: An Essay towards a Science of Education. By James Clerk Maxwell Garnett. Cambridge University Press, London, 1921. Pp. 515.

Any serious attempt on the part of an English educator to replace the traditional set of mind toward an art of education by a scientific attitude is worthy of notice. When an educator of the prominence of Maxwell Garnett (General Secretary of the League of Nations Union, and Dean of the Faculty of Technology in the Victoria University of Manchester) gives up all his spare time for eight years to the preparation of so pretentious a volume, certainly it is worth more than passing mention. To the Englishman, the book will doubtless mark a significant step toward the recognition of psychology as related to education. To the American, it indicates the systematic and cautious method by which the educators of the mother country are testing materials which, for some years, have been accepted in this country as basic. The book also shows something of the present state of mind of the typical English educator of the more advanced school.

Perhaps the best way in which to contrast the English and the American point of view is to give, without comments, the principal sources of the author's information, and the conclusions he has reached. The book is in three parts, each one of which will be briefly treated.

Book I gives three chapters to a consideration of the aims of education in the past, with a statement of the present situation. Rather startling is the statement that "the most easily observed characteristic of English education at the present time is perhaps its aimlessness." Accordingly, the author discusses a basis for determining a suitable aim toward which a science of education should be directed; Professor John Adams' conclusion that self-realization and a many-sided interest cover the whole field of educational objectives especially appeals to him, and so he finally concludes that the first aim of education during adolescence and maturity must be to build up a "single wide interest."

In Book II (16 chapters) "some of the conceptions of physiological psychology are employed in the attempt to analyse the foundations of character and their effect upon behavior." Rejecting a behavioristic view, and the doctrine of psychophysical parallelism, the author assumes "what Dr. McDougall calls the 'old common-sense view' that psychophysical interaction does in fact take place." Accordingly, in his psychological

presentation, he follows James and McDougall, except where the work of Spearman, Pearson, Hart, Cyril Burt and Edward Webb seems to give him experimental data which amplify and verify otherwise debatable conclusions.

As a result of this discussion, five "Laws of Thought" are enunciated. They are:

1. To every psychosis there corresponds a neurosis.

2. Excitement in the nervous arc tends to spread to every other arc that is connected with the first through synapses, the insulation of which the excitement in question is intense enough to overcome.

3. Any nervous arc of the higher level, if intensely excited relatively to other higher level arcs, tends to drain the impulses from those other arcs.

4. Will, measured by the general factor, "g", can reinforce the excitement in any excited system of higher level arcs; and so may cause that system to drain the excitement from all other active arcs of the higher level.

5. Action is the normal end of every train of thought.

Having arrived at these laws, and having noted that the first four laws show that a man's thought is determined, apart from incoming sense impressions, by his neurography and his will, it follows that, given a man's environment to which these incoming sense-impressions are due, his reaction will be determined when his neurography and his will are determinately known. "At least, we have no evidence of any other factors, and therefore do well to accept this view as the simplest possible hypothesis that fits all the known facts." A strong character is then made up of a neurography in the form defined as a single wide interest-system, and a strong will that cooperates with the single wide interest in guiding thought and conduct. Any one whose character is of this kind will possess an outlook on life which is something more than a philosophy, for it will show everything focussed in a supreme and dominant purpose. This purpose introduces deep emotional elements into his philosophy and transforms it into a religion, along with which he should have faith and hope..............To achieve correspondence between the central elements of one's neurography and the central essence of the endarchy of science is the most important step, on the neurographic side, towards the formation of character. One may achieve this either by accepting, provisionally, prevailing opinions, or by seeking an inspiration on one's own account. In either case, one must proceed to act on one's provisional hypothesis with a view to its verification. All conditions are satisfied by the fundamental Christian hypothesis: that God is the center of the universe, the central fact of the endarchy of science; that knowledge of God begins by faith; that hope of eternal life belongs to people who seek to know God; that brotherly love is of the very essence of God; and that all human beings are in peculiarly close relation to God. Thus, in a maximally progressive community, the common supreme purpose must be the Christian character just defined. The aim of education, the world over, follows at once: to form Christian characters, with all the manifold outward differences that are necessary if their several owners are to cooperate effectively for the fulfillment of their common supreme purpose.

Book III (7 chapters) describes a system of education to realize this aim, especially applicable to the situation in England, and sets forth plans for bringing her educational system into harmony with this aim within the next ten years. The problem is dependent primarily on knowing what each person's occupation is going to be. So it is necessary to investigate the qualities, especially the kind of "single wide interest", and the degree of "general ability or g", required by those who are to occupy the various positions in industry, commerce, and other essential departments of English life, and afterwards to indicate a means of developing the required qualities in a sufficient number of persons selected on account of their in-

nate aptitudes for each different kind of work. This selection must be made irrespective of the young peoples' place of residence and of their private financial circumstances........"We should neither ignore the value of education altogether, nor attempt to provide all men and women, whatever their ability or inclination, with identical education, but rather bring every kind of education within the reach of all citizens whose educational promise is sufficient to justify their selection to receive, if necessary at the public expense, that type of education, which, having regard to their ability and inclination, will best fit them to serve their fellows and to find their own happiness in that service." The classification of various kinds of public service is to be: (*a*) leaders in thought and action; (*b*) managers, officials, and others of intermediate rank; (*c*) craftsmen, skilled tradesmen, and leading hands; and (*d*) laborers, repetition workers, and other unskilled persons without specific training. In order to meet the demands of training for these various classes, attention should be paid to the application of the principles already laid down to methods of instruction and to types of schools. Both of these factors are touched upon at greater length than would be expected in a volume of this scope. The plan of schools includes ample provision for elementary, secondary, technical, university, graduate, and research institutions, and stresses particularly the need of part-time institutions of each grade—junior part-time, senior part-time, intermediate, advanced, and university part-time schools and classes. Ample provision is indicated for transfer from any one type of institution to another, when the right sort of ability is developed. In order to facilitate the universal opportunity, an elaborate national scholarship system is suggested, to cover every sort of training.

Finally, the volume closes with stress upon the supreme importance of training, appointing, and retaining teachers of the right kind: "in short, a perfect system of education requires, above all, perfect teachers; and perfect men and women, whatever their walk in life,—be it teaching or any other,—require a perfect education, an education that achieves its aim and so forms Christian characters."

One cannot imagine a psychologist reading Book II without reacting vigorously against certain conclusions there drawn; and one is certainly impressed no more by the matter included than by certain notable omissions. Yet the final impression is that an important advance has been made toward the end the author has had in mind, namely, the recognition in England of the applications of science to education. To the American educator, as well, there is much, especially in Book III, which is worthy of close attention.

R. H. Jordan

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PSYCHOLOGICAL PERIODICALS

Zeits. f. Psychologie. Bd. lxxxviii., Heft 1 u. 2. E. R. JAENSCH. 'Grundfragen der Akustik und Tonpsychologie:' H. Lachmund. 'ii. Vokal und Ton.' [Experiments with an improved form of Jaensch's apparatus show that pitch is most certainly eliminated and vocality induced if the 'disturbing factor' is not wholly irregular but has a certain periodicity; the best vowels appear with periodic variation both of wave-length and of amplitude. The quality of the vowel depends on the average rate of vibration; 450 is characteristic of O, about 930 of A. Periodicity in the complex wave-form may set up a voice-tone (*Stimmton*), which ousts the (higher) average-frequency tone and carries the vocal quality. —The success of Helmholtz' synthetic procedure depends mainly upon the intensive ratio of the component tones. Its results are in accord with Jaensch's theory, since it provides for a 'disturbing factor' (the higher tone) and for a voice-tone.] H. Lachmund. 'iii. Ueber die Abhängigkeit der scheinbaren Schallstärke von der subjektiven Lokalisation der Schallquelle, ein Analogon zu den sog. zentralen Faktoren des Farbensehens.' [A source of sound is placed to right or left of the median plane, and the sound is so led to the two ears that the intensity of the uniaural impressions is the same. With binaural hearing, the source is localised to right or left, as the case may be, and the sound is far louder in the 'nearer' ear. The phenomenon is analogous to the 'memory color.'] E. R. JAENSCH. 'Ueber die Vorstellungswelt der Jugendlichen und den Aufbau des intellektuellen Lebens:' P. Krellenberg. 'v. Ueber die Herausdifferenzierung der Wahrnehmungs- und Vorstellungswelt aus der originären eidetischen Einheit.' [If the image of sense-memory (*Anschauungsbild*) is the matrix out of which all perceptive and ideational experience has developed, it should be possible to discover examples of the undifferentiated *Einheitstypus*. Such individuals may, in fact, be found; they show no difference, under various experimental conditions, of after-image, image of sense-memory, memory-image; for them the eidetic state is the natural, ordinary psychophysical condition. They are very often of tetanoid constitution, and by medical treatment may be brought to imaginal dissociation. The memory-images most resistant to change from the original type are those of interesting (important, meaningful) objects and of form (as means of orientation).] J. PLASSMANN. 'Die Milchstrasse als Gegenstand der Sinneswahrnehmung.' [Aside from its astronomical and psychophysical (star-magnitudes) interest, the Milky Way offers problems to psychology both by its actual appearance and by reason of the divergence of its pictorial representations.] G. RÉVÉSZ. 'Tierpsychologische Untersuchungen: Versuche an Hühnern.' [Notes on color-contrast; on apprehension of quantities and tendencies to choice (extremes of a series are taken); on precision of pecking movements; and on formal effect of practice.] Literaturbericht.

Arch. f. d. ges. Psychologie. Bd. xli., Heft 3 u. 4. M. MOERS. 'Untersuchung über das unmittelbare Behalten bei verschiedenen Darbietungsarten und über das dabei auftretende totale und diskrete Verhalten der Aufmerksamkeit.' [Experiments with letters and meaningless syllables. (1) With visual presentation, an exposition of the stimuli at rest and spatially juxtaposed is preferable to their exposition in movement and at the same place; the former mode simplifies eye-movement (secures clearer visual images) and facilitates localization.

Auditory presentation is on the average almost as favorable as the better mode of visual. In particular, it is more advantageous for the auditory type than for the visual-auditory type; and it gives better results than visual presentation with the purely visual type, since it makes less demands upon energy. (2) The experiments confirm Meumann's distinction of total and discrete attention; the two forms admit of precise differentiation. In discrete attention, the characteristic features of immediate (as opposed to permanent) retention are strongly repressed.] R. Rudlowski. 'Kasuistischer Beitrag zur Psychologie der Aussage.' [Actual incidents are more instructive than picture-experiments. The emotional factor increases the percentage of wrong statements made under oath, and is more disturbing to women than to men.] T. Lehmann. 'Zur Psychologie des Vergleichs kurzer Zeiten.' [Experiments with short empty intervals (made by means of an ingeniously simple apparatus) confirm Katz' finding of unimembral judgments; the interval of comparison may be the sole basis of judgment, without increase of the *DL*, without any sort of conscious representation of the standard interval, and without recourse to secondary aids to comparison. This result is a matter of sensory *Einstellung*, which may be surprisingly persistent.] E. Czuber. 'Zur Theorie der linearen Korrelation.' W. Wirth. 'Bemerkungen zu der vorangehenden Abhandlung von Herrn Prof. E. Czuber über die Theorie der linearen Korrelation.' W. Wirth. 'Nachwort.' [Discussion of certain points in Wirth's "Spezielle psychologische Massmethoden" (Abderhalden's *Handbuch der biologischen Arbeitsmethoden*, 1920).] M. Tittel. 'Ueber Angleichung und Kontrast im Tongebiet.' [Determination of the *DL* of successive fork-tones, when the standard (*N*) is preceded by an inducing (*I*) stimulus. Assimilation occurs when the interval *I-N* is less, contrast when it is greater than an octave. (The one exception to this rule is the appearance of assimilation in place of contrast with musical *Os* and pure consonant intervals.) The assimilative effect is greater than that of contrast; it is greatest with neighboring *N* and *V* (second, minor third), least with the intervals of fourth and fifth. The effect of contrast in its sphere (large, mistuned intervals) is approximately the same for all intervals. The absolute magnitude of the induction-effect increases with rise of pitch-number. Induction is centrally conditioned.] A. Fischer. 'Alexius Meinong.' A. Berliner. 'Bestimmung der Zuverlässigkeit bei der Methode der relativen Stellung mit besonderer Berücksichtigung der Werbeforschung.' [Formulas and illustrations.] A. Berliner. 'Zusammenhang zwischen aesthetischem Wert und Wiedererkennen.' [Experiments on the rank-order of postcard pictures show that the aesthetically preferred members of a group are also the more easily recognized.]

Arch. f. d. ges. Psychologie. Bd. xlii., Heft 1. u. 2. E. Berner. 'Allgemeine Untersuchung der zwischensubjektischen Beziehungen bei den neueren deutschen Skeptikern.' [The problem of intersubjective. relations has received four typical solutions: the negative or solipsistic (Schubert-Soldern, Keibel, Ziehen); the positive by way of a real external world (Goering, Schubert-Soldern, Schmidt); the positive by way of objective assumption of an external world (Bergmann, Weishaupt, Cornelius); and the sceptical (Heim, Spir, Im. Fichte, Stirner, Nietzsche).] K. Korniloff. 'Dynamometrische Methode zur Untersuchung der Reaktionen, [Bulb-key, manometer and kymograph are introduced into the chronoscope circuit. In the muscular reaction, short time goes with increased energy and increase of the numbers (Isserlin's constants) expressing the form of movement; in the sensory reaction these relations are reversed. In general, the more complicated the activity of thought, the less intensive is the outward manifestation of the voluntary action.] O. Klemm. 'Ueber die Korrelation verschiedenartiger Auffassungsleistungen bei Eignungsprüfungen.' [Tables, with brief discussion, of correlations between range

of attention, immediate retention, counting of irregular point-groups, and reading-off of colors and color-names.] J. Lindworsky. 'Beiträge zur Lehre von den Vorstellungen.' [(1) The spontaneous arousal of palpable ideas in the course of a thought-experience is due to a pause in (or retardation of) the thought. This fact may mean that thought is in general dependent upon such ideas *in statu nascendi*, but moves too quickly for their realisation. (2) The usual, perhaps the normal, mode of development of ideas is from general to particular.] W. Möhrke. 'Beitrag zur Untersuchung der Schmerzempfindung.' [(1) Experimentally produced pain has no effect on the performance of even difficult mental tasks. (2) Adaptation to pain appears under all forms of electrical stimulation: oscillating and constant direct current and faradisation. The pain-quality under the oscillating current is dull and boring; under the constant current, cutting. The oscillating current affects the pressure organs and induces anaesthesia; the constant current hardly affects the pressure organs but arouses warmth. Adaptation is a specific effect of electrical stimulation, and may be explained on Braun's theory.] F. Nicolai. 'Experimentelle Untersuchungen über das Haften von Gesichtseindrücken und dessen zeitlichen Verlauf.' [Experiments on children and unintellectual adults show that, after a single exposure of a group of familiar visual objects, there is at first much forgetfulness but later (up to periods of four weeks) a marked return to memory. Repeated reproductions serve involuntarily to fix the ideas in memory; an immediate reproduction is of especial effect. As the number of objects increases, the span of memory also increases, tending to a maximum. If a large number of objects are presented in successive groups there is confusion, not as regards the number retained but as regards their localisation.] E. Becher. 'Benno Erdmann.' Gesellschaft für experimentelle Psychologie: Ausschuss für angewandte Psychologie. W. Wirth, W. Engelmann. 'Alfred Lehmann.'

Arch.f.d.ges.Psychologie. Bd. xlii., Heft 3 u. 4. M. Kiefer. 'Experimentelle Untersuchung über die quantitativen und qualitativen Beziehungen der monauralen und binauralen Schalleindrücke, sowie deren Verwertung zur Deutung des Weber-Fechnerschen Gesetzes.' [Reports experiments with the gravity phonometer, monaural and binaural, undertaken to determine the locus of the logarithm in Fechner's formula for Weber's Law. Monaural and binaural impressions differ in quality and in localisation; intensity and quality depend on attention, which is always locally directed; localisation of the source of sound may be disturbing, as correcting judgments of intensity. The relative *DL* is smaller binaurally, with high intensity of stimulus; with low intensity, the monaural and binaural *DL* are about the same. The binaural *RL* is always lower than the monaural. The ratio of R-intensities, binaural and monaural, which gives subjectively equal sounds, is (weak) 1:2.46, (strong), 1:5.49. As regards Weber's Law, the results are ambiguous.] E. Mally. 'Ueber die Bedeutung des Bravais-Pearsonschen Korrelationskoeffizienten.' [Derives the measure of correlation from the concept of the "elementary case of connected variations."—W. Wirth adds a critical note.] N. von Mayendorf. 'Der Sehhügelstiel des inneren Kniehöckers und seine physiologische Bedeutung.' [The fibres mediate reflex connection between excitations of the cochlearis and the cortical area for bodily sensitivity (start on hearing of sudden noise, shudder at squeak of blackboard chalk, etc.).] A. Berliner. 'Reduktion der mittleren Verschiebung bei der Methode der relativen Stellung.' [In the arrangement by rank-order the various measures of distribution are reduced by division by n (or by a value which with infinitely large n approximates infinitely closely to n) to an expression which is independent of n, the number of elements.—W. Wirth adds a critical note.] R. H. Goldschmidt. 'Rückblick auf Nachbildtheorien bis zur Herausbildung der Fechner-Helmholtzschen Auffassung.' [Discusses in particular

the fatigue-theory of Scherffer and Chevreul, the oscillation-theory of Godart, Brewster and Plateau, and the work of Aubert. In general the after-image has been regarded and studied as of peripheral origin; there are, however, many features of it—mode of appearance (Katz), general structure (Jaensch), form as compared with original, sharpness of outline, details, etc.—which point to central conditions.] R. H. PEDERSEN. 'Alfred Lehmann.' [Appreciation, with portrait.] K. GNEISSE. 'Die Entstehung der Gestaltvorstellungen, unter besonderer Berücksichtigung neurerer Untersuchungen von kriegsbeschädigten Seelenblinden.' [Utilises the work of Gelb and Goldstein, Fuchs and Poppelreuter for a critical examination of three theories of form: those of Wertheimer, of Linke, and of Meinong and his pupils. Decides in favor of the Austrian school.] S. FISCHER. 'Ueber das Entstehen und Verstehen von Namen, mit einem Beitrage zur Lehre von den transkortikalen Aphasien, i.' [Reports experiments with meaningless linear figures and photographs of unknown persons, to which meaningless names of one, two and three syllables were (for the most part auditorily) attached. After discussing the experiences of impression (*Einprägung*), the author seeks to determine the conditions under which the sound assumes a nominal function. It is important that the object be logically determinate (manifest a 'structure') and be apprehended as important (useful, significant), and that the sound be apprehended as a form or complex.]

Psychological Review. Vol. xxviii., no. 2. S. I. FRANZ. 'Cerebral-mental Relations.' [While mental alterations accompany cerebral lesions, there may be subsequent return to a normal (or nearly normal) mental state without corresponding recovery of normal brain-condition; we do not observe a definite dependence of a special mental state on the integrity of certain special cerebral parts. A better physiological psychology is sorely needed.] L. L. BERNARD. 'The Misuse of Instinct in the Social Sciences.' [An instinct is a specific inherited action-pattern. Thus it is wrong to apply the term to habit-complexes, to think of instinct as involving a conscious element, to make purposiveness a characteristic of it, to seek to define it in terms of the function of the act. The real task of the educational and social psychologist is to discover the mechanisms whereby child and citizen build up their habits, directly and indirectly, on the basis of instincts, and whereby one habit or set of habits is transformed into another.] J. R. KANTOR. 'An Attempt toward a Naturalistic Description of Emotions, ii.' [Emotions are not of general and necessary utility to the organism; are not related to instincts; as no-response actions, cannot readily be classified; are seldom if ever found in animals and young children; do not admit the dichotomy of emotional act and expression. Their conditions are constitutional (equipment of response-patterns, speed of reaction, etc.) and stimulational (familiarity with stimulus, setting of stimulus, etc.).] E. L. THORNDIKE. 'On the Organization of Intellect.' [Results of 15 tests of intelligence given to about 800 soldiers, of 7 tests of intellect and skill given to over 900, and of 9 tests of intellect given to 653 individuals, are adverse to Spearman's theory in any strict form. Further work of detail is suggested.]

NOTES

Note on Plethysmographic Technique

Inasmuch as the regular rubber sleeves for the Lehmann plethysmograph are not yet procurable, and as the American manufacturers are no longer making bladder spinal ice-bags, which proved to be an admirable substitute, one has had to use rubber gloves for this sort of experiment. It is worth noting that elbow-length obstetrical rubber gloves are now available for the first time since the war. Rubber gloves have proved to be relatively unsatisfactory for this work, owing to the difficulty with which the hand is inserted and withdrawn. Two expedients have been used: either the hand and arm have been covered with vaseline, or the hand has been inserted in the glove which has subsequently been attached to the metal sleeve. These inconveniences may be obviated by the following simple trick. Attach the rubber glove to the metal sleeve in the usual way. Then turn off the stopcock through which the water is to be inserted and, by applying the mouth to the top of the glass tube, draw out all the air between the rubber glove and the metal sleeve. This proceeding distends the rubber glove, so that the hand may be readily inserted. It has also been found advisable to draw out the air at the end of experimentation, after the water has been removed, to enable the subject to withdraw the hand easily from the apparatus.

S. W. Fernberger

University of Pennsylvania

"The Physical Growth of Children"

I am indebted to Professor L. B. Hoisington for his careful analytical review, in the April number of this Journal, of my recent Study, *The Physical Growth of Children from Birth to Maturity.*

The printing of the Study proved difficult for the local printer with limited facilities. I take this opportunity to correct some of the inconsistencies emphasized in the review by Professor Hoisington, since they are also of psychological significance to those interested in typesetting and proof-reading. "Left" was printed for "right" (p. 21); "girls" for "boys" (p. 147); and "above" for "below" (p. 192).

Bird T. Baldwin

University of Iowa

A Note on "Affirmation and Negation"

A word of critical comment on the interesting experiments of Dr. C. H. Griffits reported in the January, 1922 number of this Journal may not be amiss. The purpose of the experiments was to disprove the common opinion, as represented by Professor Breese and others, that the difference between judgments of affirmation and of negation is a logical rather than a psychological one,—that all judgments are, from a psychological point of view, affirmations.

The results of the experiments undoubtedly show that it takes more time to form a negative judgment than it does to establish an affirmative one; but is this not after all what would be supposed from ordinary observation? To infer further from this, however, that consequently there is a psychological *opposition* between a logical affirmation and a logical nega-

tion is hardly warranted. To judge that "the table is not round", "this color is not red", "these letters are not present", etc., no doubt ordinarily requires a longer time than that needed to make the respective opposite judgments; but, after all, the psychological attitude taken toward a roundness or a redness disbelieved in is the same in kind as that taken toward a not-roundness or a not-redness believed in, however much the time necessary to arrive at these attitudes may differ in duration in the two cases. In other words, the only *opposition* between affirmation and negation seems to be a *logical* opposition: the only *psychological* difference is a difference in *duration* of process, not an opposition at all. I do not see, therefore, that Dr. Griffits has proved his point as a result of his experiments.

J. S. MOORE

Western Reserve University

THE "ELEMENTS OF FOLK PSYCHOLOGY"

The publishers of the English Translation of Wundt's *Elemente der Völkerpsychologie* tell me that the words "Revised edition April 1921" which appear in the new issue are due to a mistake made by their printing department and will be removed from all copies now in stock. My remarks in this JOURNAL, xxxiii., 1922, 150 ff. must therefore be taken as belated comments on the original edition.

E. B. T.

URBAN'S TABLES YET AGAIN!

Dr. G. J. Rich points out that the value 2.2365 in the last line of p. 303 *sup.* should be 2.2363.

AUGUSTUS DÉSIRÉ WALLER

Professor A. D. Waller, since 1902 director of the physiological laboratory of the University of London, and for many years an associate editor of this JOURNAL, died on March 11, at the age of sixty-five. Waller's physiological researches covered a wide range (see *Nature*, cix., April 1, 1922, 418 f.). He is best known to psychologists by his studies of the sense of effort (*Brain*, 1891) and of the psychogalvanic reflex (*Proc. Roy. Soc.*, 90 B, 1917-19, etc.). In 1891 he published *An Introduction to Human Physiology*, which showed a keen interest in psychophysical problems, and in 1912 he ventured a *Psychology of Logic*.

APPOINTMENTS

At Harvard University, Dr. Herbert S. Langfeld and Dr. Edwin G. Boring have been appointed associate professors of psychology and Dr. Carroll C. Pratt instructor in psychology. Dr. Langfeld is promoted from an assistant professorship at Harvard. Dr. Boring has been professor of experimental psychology since 1919, and Dr. Pratt instructor in experimental psychology since 1921, at Clark University. The psychological staff at Harvard will consist of Professors McDougall and Dearborn, Associate Professors Langfeld and Boring, Dr. Troland, and Dr. Pratt.

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FILM, SURFACE, AND BULKY COLORS AND THEIR INTERMEDIATES[1]

By Mabel F. Martin

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I. Introduction

Katz has made us familiar with a mass of detail concerning the various modes of appearance of colors, which were first hinted at by Hering.[2] There is a general tendency among psychologists to accept Katz' detail, but there seems to have been no definite effort made to confirm it from the foundation. This work of verification, desirable in itself, is all the more important because Katz' interest lay not so much in phenomenology for its own sake as in other and more special problems. For these reasons we have attempted a further study of those modes of appearance which seem fundamental.

[1]From the Psychological Laboratory of Cornell University.

[2]D. Katz, *Die Erscheinungsweisen der Farben*, 1911.

In the opening chapter of his book Katz describes and compares eight modes of appearance: (1) film colors (*Flächenfarben*),[3] (2) surface colors (*Oberflächenfarben*), (3) transparent plane colors (*durchsichtige Flächen*), (4) bulky colors (*Raumfarben*), (5) mirrored colors (*gespiegelte Farben*), (6) lustre (*Glanz*), (7) luminosity (*Leuchten*), and (8) glow (*Glühen*). Film colors, surface colors, and bulky colors are the most important, because all other modes of appearance may be regarded as forms or combinations of these.[4]

In comparing surfaces and films, Katz takes paper as a typical example of surface, and the colors seen in a spectroscope as a typical example of film, although many other examples are also given. The observable differences between film colors and surface colors he groups under five headings: localization, texture, orientation, configuration, and aesthetic effect.[5]

1. *Localization*. "The spectral color of the usual apparatus is not localized with the same definiteness at a precisely determinable distance from the observer as the color of the paper." In saying that the film color is indefinitely localized, Katz does not mean that its distance varies from moment to moment. He says emphatically that, whether the observation is continuous or recurrent, with constant *Einstellung* of the eyes the distance of the color in no way changes. By indefinite localization he means positively indeterminate. It is true that probable limits may be set; but these limits vary with the *perceptive* conditions under which the film color is seen. For example, the limits lie much farther apart for the film of the sky than for that of the color seen in the spectroscope. Within these extraneous limits, the localization is positively indeterminate.

2. *Texture*. There is a baffling contradiction about the appearance of the film color. It seems to suggest depth, but proves in fact to be impenetrable. "The paper has a surface in which its color lies. The plane in which the spectral color stretches through space before the observer does not possess a surface in the same sense."[6] One almost feels able to penetrate more or less deeply into the spectral color, whereas with the color of paper the gaze is forced to stop at the surface. The openness, the softness, the insubstantiality which the spectral color betrays is, however, not of such a kind that one can speak of a clearly bulky mode of appearance of the spectral color, that is, of a visible filling of space in three dimensions, or of any sort of colored transparency. Like the color of paper, the spectral color is bidimensional and screens the space behind from view.

3. *Orientation*. There is a marked difference in the orientation of the two types of color. The surface in which the color of an object, such as paper, seems to lie can take any orientation to the line of regard. The film color tends always to assume a frontal-parallel position. Indirect vision of course constitutes an exception to this rule, and there are certain other exceptions. For example, the apparent orientation of film colors is greatly influenced by the orientation of neighboring objects of *perception*. Katz describes an experimental arrangement whereby a bit of the sky, viewed through a cardboard tipped at an angle, takes on an apparent

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[3]We could find no entirely satisfactory term for the appearance of the "completely reduced" color. Katz' word, *Fläche*, does not suggest an English equivalent. After trial of several words and after consultation with our *Os* we decided upon the term "film." Our "film," however, as is shown in the text, is not absolutely identical with Katz' *Fläche*.

[4]*Ibid.*, 6.

[5]*Ibid.*, 7.

[6]*Ibid.*, 7.

orientation about midway between that of the cardboard and the usual frontal-parallel position.[7] In spite of such susceptibility to secondary influences, the film color possesses in and of itself a tendency to appear in frontal-parallel orientation.

4. *Configuration.* The surface of a body may be either smooth or ridged and curved in various ways. The film color on the contrary always lies in a practically smooth flat plane.[8]

5. *Aesthetic Effect.* The spectral color has something delicate about it, something more pleasing aesthetically, than the color of paper.—

This completes Katz' list of differences between film colors and surface colors. In connection with the third difference, that of orientation, Katz discusses, however, another difference, which he seems to regard as more fundamental than any of these five. Surface colors not only conform to the surfaces of objects, but they seem also actually to be stable properties of the objects. We can shadow or illuminate one and the same surface. A shadowed or illuminated film, however, is simply a new film color. Surface colors are color-qualities of objects. Film colors are unreferred colors, colors as such.

"So far as the psychological conditions are concerned, the consciousness of having an object before one on which the colors arise is for the perception of surface colors, I venture to say, of decisive significance...... As compared with the great influence which the inner *Einstellung* exerts on the occurrence of this or that mode of appearance of colors, one may designate as almost irrelevant the physical source of the radiation setting up the impression."[9]

The relations between film colors and surface colors can easily be studied in the laboratory, because any sort of surface color can be reduced to film color by the use of a screen with a small hole, or (still better) of a double-screen.[10] The exact form of double screen is not important provided the following essential conditions are satisfied. (1) The screen must cover the object completely except for the part seen through the opening. (2) At the same time it must not permit any structure or grain that may possibly be present to be recognized. (3) It must not permit any nonfrontal orientation of the portion of surface seen to be recognized. (4) The hole must not be too large.

Between surface colors and film colors, all possible phenomenological transitions occur.[11] Many *O*s experience such a transition when monocular observation is substituted for the usual binocular vision. An intermediate which approaches more nearly to the true film can be secured by placing before the eye a lens too strong to be overcome by accommodation.—

The third of the essential modes of appearance of colors is that of the bulky colors. These differ from film and surface alike in that they are tridimensional and partially transparent. They seem to fill a *definite space* in its three dimensions. "According to my observation," says Katz, "they show this property in a distinct way only when they are at the same time in a true sense transparent," that is, so long as objects are actually seen through them.[12] When no object can be distinguished through the color, Katz would call it a film color.

Bulky colors, like surface colors, can be reduced to film colors. For this reason, Katz considers the film as the original mode of appearance of colors, and regards all other modes as derived from the film.

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[7] *Ibid.*, 75.

[8] *Ibid.*, 12.

[9] *Ibid.*, 9.

[10] *Ibid.*, 9 f.

[11] *Ibid.*, 9.

[12] *Ibid.*, 17.

The theory of Katz whose implication is, evidently, that the film represents the ultimate stuff of vision. Color or light in the psychological sense is film. If we assume with Katz that the film is preperceptive, it follows that localization, orientation, and configuration cannot be attributed to it as such, and appear under experimental conditions only because and in so far as secondary perceptive conditions are introduced by the experimental arrangement. Taking over Katz' empirical statements, we might lay down the following laws: (1) The film is not localizable. If it is localized, that is merely because, e. g., we are familiar with the instrument through which we are observing. (2) The film has no orientation. If it appears always in a frontal-parallel position to us that is simply because, in thinking of it as object of observation, we give it the easiest and most obvious orientation, taking it, so to speak, on or across the line of regard. (3) The film has no configuration. As sheer sensory quality, it must show as a phase of homogeneous color.

Before the psychological status of the film is fully established, these statements must be confirmed experimentally by other *Os*. Psychologists have perhaps tended to take Katz' statements to be systematic; they are, in fact, merely empirical. The reader of the introductory chapter can scarcely help noticing many apparent contradictions. If, *e. g.*, Katz is correct in assuming that the fundamental difference between surface and film colors is that surface colors are ascribed to objects and film colors are not, in what sense can he assert the existence of transitions between film and surface? Surely the source ascribed to the color either is or is not an object.

Our experimental series were directed toward answering the following questions: (1) Are there intermediates between film and surface? What is their phenomenology, and in what sense may they be regarded as intermediates? (2) Can it then be shown experimentally that the object-consciousness is sufficient of itself to ensure the perception of surface color? (3) If the film color really is the ultimate stuff of vision, what happens to it that changes it to surface color in the one case and to bulk in the other?

The *Os* were Dr. L. B. Hoisington (H), assistant professor of psychology; Dr. H. G. Bishop (Bi), instructor in psychology; Miss C. C. Braddock (Br) and Miss G. Adams (A), graduate students in psychology; and the writer (M). All had had training as *Os* in previous psychological investigations.

The apparatus used for reducing surface to film was a double screen, similar in essential details to that described by Katz. It consisted of two entirely separate screens, constructed as follows. To a wooden frame, 71 cm. x 56.5 cm., was tacked a sheet of heavy grey cardboard which approximately matched in brightness no. 10 of the Hering series of grey papers. In the cardboard was a circular hole, 25 cm. in diam., whose center was approximately 31.5 cm. from the bottom of the screen, 25 cm. from the top, and 35.5 cm. from each side. To the back of each screen was fastened a strip of wood so cut that it formed a slot into which could be dropped smaller sheets of the grey cardboard. In the center of these smaller screens was a hole. The holes varied from 2 cm. to 20 cm. by steps of 2 cm., and were numbered from 0 to 9. There was a pair of every size, one to fit each of the two members of the double screen. The back of each hole was ground down with emery paper to present a sharp, knife-like edge. When one of the small screens was fitted into the slot, the small hole was concentric with the large hole.

The experiments were performed in a dark room, approximately 6 x 2.5 m. It was lighted from above by three 'daylight' lamps, arranged in a single row along the middle of the ceiling. The table to which the apparatus was fastened was set up at the side of the room, almost exactly midway between two of the overhead lamps, so that the screens and the stimulus behind them received an almost equal amount of light from the two sides.

There was practically no brightness-contrast between the fore and back screens in these experiments. The screens, being entirely separate, could be set at any desired distance from each other, but in all regular series they were 25 cm. apart. Other distances which might be varied were the distance of the stimulus from the back screen and the distance of the *O* from the fore screen.

From a few preliminary trials it seemed probable that reduction is favored by increased distance of *O* from the fore screen and by increased distance of the stimulus from the back screen, just as it is favored by a relatively wide distance between the two screens themselves. In all regular series, the *O* sat at a distance of 1 m. from the fore screen. This position was kept constant by the use of a fixed headrest.

II. Surface to Film

Series I. Effect of Varying Size of Hole

Series I was originally planned to answer our first question: Are there intermediates between film and surface?

The colored stimuli were cloths stretched very smooth on wooden frames, and Milton Bradley papers pasted on sheets of stiff cardboard. These could be fastened by thumb-tacks to a wooden and cardboard frame, so that a quick and easy shift from one color to another was possible.

When viewed without the double screen, these colored stimuli were, of course, ordinary surfaces. When they were viewed through the very small holes, their appearance approximated in most respects to the film as described by Katz. There were certain differences, to be discussed in detail later, but these need not concern us here. It seemed plausible to suppose that by varying the size of the holes, we should find, somewhere between the largest holes, which gave surface, and the smallest, which gave an approximation to film, some of those intermediates to which Katz refers.

The surfaces, like the films to which they were to be reduced, were flat and possessed a frontal-parallel orientation. The criteria of the true film were assumed to be indefinite (*i.e.*, only secondarily motived) localization; bidimensionality; loose, soft, insubstantial texture; and complete lack of objective reference. We purposely omitted aesthetic effect, because we could not agree with Katz in regarding this as an observable difference on the same level as the others. All *O*s, however, occasionally exclaimed at the beauty of the film colors, in spite of the fact that aesthetic effect was not mentioned in the instructions.

It will be remembered that Katz' *O*s fixated the edge of the hole in the back screen,[13] because this fixation was most natural and convenient and was moreover adequate to the purposes of his experiments. Since, however, we were to ask for reports of localization and dimensionality, it seemed best to instruct the *O*s to fixate the color under observation.

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[13] *Ibid.*, 38, 73, etc.

[illegible]

[illegible]

Group I. Surface Colours

The phenomenology of surface colours presented no real difficulties. The descriptions were simple, brief, and consistent.

Localization. [illegible] "About a foot and a half behind the screen." [illegible] "[illegible] from the screen." Br (8) "Localised at a slight distance behind the back screen." H (9) "Surface behind the screen at some distance, a meter or so." H (9) "About 5 m. behind [illegible]." M (7) "Surface quite definitely localized behind the back [illegible] screen, though I can't give the exact distance in feet and inches."

Objectivity. [illegible] "I could make out the inequality of the cloth, the [illegible]." A (9) "Rough green cloth." Br (8) "More uniform than the [illegible]." H (7) "Fairly definite object, cloth, or paper, [illegible] with a grain to it." M (9) "Opaque soft cloth."

Texture. [illegible] "Weave of cloth." A (8) "Rough. I can see ridges, [illegible] together, blurred." Br (7) "Smooth shiny cloth with darkish

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[illegible] in parentheses refer to the number of the hole used.

grain." H (9) "Coarse woven cloth, slightly fleeced, so that texture is just a little obscured. Solid. Opaque." M (7) "Solid, opaque, soft cloth with ridged weave."

Dimensionality. **Surfaces were always reported as bidimensional.**

Group II. Film Colors

The *O*s were unanimous in describing the film color as indefinitely localized; but as regards the stability of the localization of film colors we obtained results that at first seemed opposed to those of Katz. It will be remembered that he combats the contention of Hillebrand that indefinite localization involves shift of position,[15] and asserts that the film color does not change its distance, although it is positively indefinite in localization. Hence we were surprised to get occasional results like the following.

H (3) "One can do stunts to those things, so far as localization is concerned. They are instable. One can read all kinds of meanings into them." H (2) "Localization is shifty. At first it seemed to fill all the space behind the screens; but I took a localization attitude later, and it was easy to throw the thing farther back." H (1) "Slightly shifting localization. I don't mean that at any time I could say just where it was, only that sometimes it seemed nearer and sometimes farther." H (4) "Localization tends to shift from at the screen to far behind." Br (4) "It lay behind the back screen, came forward a bit, and then seemed to shift up to the fore screen." Bi (0) "I'm not sure whether it was always in the same place or not. I think there was a little play back and front. Not much."

The opposition between these results and those of Katz was, however, apparent rather than real. Katz says that the localization of the film color is stable with *constant Einstellung of the eyes*. With a little more practice our *O*s soon discovered that the apparent shifts in localization were due to shifts in fixation. It will be remembered that the original instructions read "Fixate the center of the disc." These instructions in a sense demanded the impossible, because there was nothing there to fixate.

H (1) "When you try to fixate, you have just that meaning of looking for something you can't see. You tend to stare. The eyes are held at an immense strain." Bi (6) "Almost impossible to fixate the center of the disc. My accommodation is for two or three different places!"

In hunting as it were for a place to anchor, the fixation naturally shifted about, and the film color, having no localization of its own, followed the changes of convergence. It therefore seemed advisable to alter the instructions with respect to fixation to read: "Regard the white spot at the lefthand edge of the screen opening as a fixation point and begin every observation with your eyes directed to that. You need not, however, feel obliged to maintain this fixation throughout the period of exposure." Under these instructions the *O*s made more accurate reports of their fixation, and it was found that apparent shifts in the localization of the color were always correlated with shifts of fixation.

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[15] *Ibid.*, 11.

[illegible]

In regard to the tendency of the unlocalized film color to link itself with perceptive experiences in the visual field, our results also agree with those of Katz.

H [illegible] "It's just out there, apparently localized only in connection with some other experience." H [illegible] "Meaning of fixing the gaze at nothing in particular. In that sense you may say that the thing is not localized at all. But even so, one must fixate the gaze beyond the second screen and that seems to carry the meaning that the color is beyond that. One can't fixate the eyes at a point and say the color is there. In that sense the localization is somewhat indefinite." Br [illegible] "When I fixate the edge of the disc, I can see that the brown is behind that edge somewhere. No front or back limit to the brown."

In certain preliminary experiments, in which we tried to determine what length of exposure was most convenient for observation, we found that this tendency of the film color to link itself to the nearest perceptive object, the screen, is enhanced by very brief exposure times.

H "With the shorter exposures, the tendency for the color to be linked up with the back screen is a little more pronounced. With the 1 sec. exposure, it became almost a part of the screen, only it had no definite surface, nor any objective reference; but it belonged to the same depth level."

Thus our results with regard to the localization of the film colors are in full agreement with those of Katz. This was not the case with dimensionality. The colors observed were not unequivocally bidimensional.

H (1) "Comes right up to the screen, but extends back indefinitely." H (2) "Depth, but not penetrable." Br (1 B)[14] "I couldn't look through it. It wasn't gauzy, but it seemed to have a bulkiness about it. I shouldn't say definitely tridimensional. Indefinitely so." H (1) "Tridimensional?

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[14] Letters following the numbers in parentheses refer to the colored stimuli. When no letter appears, a dark brown rep was used as a stimulus. Other backgrounds used in experiments from which these introspections are quoted were bright green cheesecloth (G), red-orange cheesecloth (O), purple lawn (P), and light brown gingham (B).

Hard to say. It might have been an unlocalized bidimensional something. The fact of its not having a beginning is the reason for calling it tridimensional. It may be due precisely to the fact that it was not localized." H (1) "I fixated the white spot practically all through the exposure. Under those conditions, I should call the color bidimensional. At least I didn't see any tridimensionality." M (1) "Not definitely tridimensional nor definitely bidimensional. I should have been inclined to call it tridimensional, but could not do so, because it has neither front, back, nor middle. My reason for wanting to call it tridimensional is that it is not localized in a single plane; but calling it tridimensional would not really help because it is not localized in any bulk of space either." M (1) "Tridimensional in the sense that very dense fog and thick, non-transparent dust clouds are tridimensional. Certainly not tridimensional in the sense that fluids in bottles or masses of jelly or blocks of colored glass are tridimensional. The brown color here had no beginning and no ending in the third dimension." A (1P) "I think I'd call it three dimensions, but I'd have to stretch what I mean by that. It certainly is more than two dimensions." A (1P) "I wouldn't say it was definitely in three dimensions. It was very indefinite. It's different from what I'm in the habit of calling dimensions. Dimensions are usually either solid or they've got some boundary that's flat or that blocks the thing off in some way. This sticks out in front, but it sticks out in such a vague way. It's not solid, and it hasn't any boundary." Bi (oB) "Not sure about the dimensionality, beyond the fact that it was spread out." A (oG) "I can't localize it, and I can't tell about its dimensions either." H (1) "I don't know. It certainly both is and isn't tridimensional, if one can make any sense out of that. Bidimensional, I believe." H (1) "One is left with the meaning that you don't know whether the thing is tridimensional or not." H (1) "How in the world can you say anything about the dimensionality of a thing if you don't know where it is?" H (1) "Bidimensional. I believe that the difficulty with the dimensionality is one of localization primarily." M (1B) "Bidimensional in the sense of having no definite extent in the third dimension, but tridimensional in the sense of not being localized precisely in one plane."

From these and similar observations it would appear that the phenomena with which we were dealing were neither bidimensional nor tridimensional. They were merely extended, without dimensions in the geometrical sense. We might almost have anticipated this result. For, after all, dimensionality in that sense is already a *perceptive* character. If a color is localized in one plane we call it bidimensional; if it is localized in more than one plane, we call it tridimensional; but if it is not localized at all, on what basis can we call it either? In describing the color as bidimensional, the *O*s had reference to its smoothness and impenetrability; in describing it as tridimensional, they had reference to its softness, looseness, and "invitation to penetrate." In either case, what the *O*s were describing was not dimensionality proper, but the *predimensional* nature of visual quality. So in the light of our results it seems evident that the product of the "reduction" of surfaces cannot be called bidimensional in precisely the same sense that a surface color is bidimensional. The film is bidimensional in the negative sense, in that it is not yet positively tridimensional. It is also tridimensional in the negative sense, in that it is not yet positively bidimensional.

The question of objective reference gave no difficulty. All the *Os* reported that the pure film colors had absolutely no objective reference.

The film colors had very characteristic textures, but the *Os* found considerable difficulty in describing these except by the use of perceptive analogies.

A (1) "It's hazy, but not haze. In the woods where there's been a fire and smoke lies in a thin layer on the ground you'd have something like this." H (1) "Like something viscous or gelatinous, if you could see such a thing without a definite front face." H (3) "Haze, solid, translucent, tri-dimensional,—like colored glass,—and yet, no! No analogy under the sun is exactly correct. The light is what bothers me. I don't see light through it nor light reflected from surface, nor is the thing especially luminous." Bi (oP) "It had a curious soft penetrableness." A (oG) "You wouldn't say it's luminous, but there's light in it in some way. It wasn't reflected from it." H (general observation) "The darker ones are always more like looking into a colored hole, whereas the brighter ones seem glowing." M (1) "Opaque in the sense of shutting off space behind, but not opaque in the way that a solid is opaque, for the shutting off did not occur at any particular place. I could look into the color, but not through it." M (1) "It was not opaque as solids are opaque. Yet it could not in any sense be seen through. The meaning of penetrability seems to be associated with the effort to gauge localization." Bi (5 O) "Self-radiant, somehow. I'm sure that if I didn't force it down by careful fixation it would be a tremendously glowing color. It's something like the glare we get at night when we approach automobile lights. It has the same beaming, dense character." Bi (3) "Haze points too much toward fogginess. This is beautifully clear. This is too dense for thin smoke............It's like looking into a window from a lighted room into the dark. The dark comes clear up to the window, but you don't know where it leaves off. Same kind of thing here exactly." Bi (2) "No texture in the usual sense. Foglike, but not so dense as fog. Localization is uncertain. It seemed to stand behind the hole in space. There is a penetrability about a fog that this does not have." H (1 O) "I don't know whether it's translucent or not. It's something like translucent. Rather like dense gas, though not exactly like any I've ever seen." H (oP) "It's just a patch of color that isn't objective, and yet one tries to apply textural terms to it. If you have a texture, it must be a texture of something."

While in general there is no harm in saying that the tissue of the film color is soft and insubstantial, our reports indicate very clearly that the film is *pretextural* in the same sense in which it is predimensional. Texture is a perceptive character, and therefore cannot in its ordinary sense be attributed to a sensory quality. Texture, like dimensionality, seems thus to be bound up in some way with fixation.

H (3) "It's easy for that to be two different things: (1) a transparent (I don't like the word glassy) thing that becomes gradually less translucent, and then gets so dense that you can't see any farther into it. The thing shifts from that to (2) something at or almost on the screen, lustrous, almost luminous. I have never seen anything like it. I'm certain these two things depend on a shift of fixation."

Intermediates

No true psychological intermediates were found, under the experimental conditions of this series. With the holes inter-

mediate in size between those giving ordinary surface and those giving pure film, we obtained appearances that involved both simultaneously. These were usually described as a surface, seen more or less obscurely, and more or less definitely localized behind a semi-transparent medium of the same color. Similar appearances were frequently obtained with holes that at other times under precisely the same external conditions gave ordinary surface on the one hand, or pure film on the other.

The phenomenological intermediates found with different colored stimuli and various sizes of holes differed in the localization and texture of the surfaces and in the depth and density of the obscuring media. Strictly speaking, they did not fall into sharply defined classes. For convenience, however, we have classified the results more or less arbitrarily into four fairly distinct groups. Starting with surface as given, we have the following.

Group A. Soft Surface

Br (5) "More like surface than anything else, a surface that is a little thick; like a woolly surface, loose-woven so that you can look into it; like thick, blankety cloth." M (9B) "Cloth. Bidimensional. Definitely localized behind screen at uncertain distance. Very soft, velvety surface." H (7) "Solid without being highly resistant, like cut liver. Velvety comes nearest, but—! Something that by vision shows it is soft to touch. Bidimensional with a hint of tridimensionality." A (7) "Like a woolen sweater or a chinchilla coat or soft snow." Bi (9) "Nearly surface. The color is one foot or more behind the screen. Surface is irregular mottling of color."

Group B. Veiled Surface

Br (8) "Surface with a slight film over it." Bi (7) "Surface, 1 ft. behind screen. Out in front there was a little bit of brown haze." Br (8) "I don't know where the front or back of the film lies, but I do know where the surface component is." A (6) "As if you had a surface color and a haze in front of it." M (7) "Plainly cloth, not quite definitely localized. Suggestion of very thin, indefinitely localized film of brown haze somewhere in front, perhaps contiguous with the surface, perhaps detached from it. The cloth itself appears velvety, *i. e.*, the color is not localized in one plane, and still less is there any distinction of localization of separate threads to form a clean-cut cloth texture." H (7) "Very thin cloud or gauze before a solid, opaque, slightly irregular surface."

Group C. Fogged Surface

M (6) "Smooth, stony surface. It might be cardboard, stone, wallpaper, or cloth. The brightness pattern suggests cloth, but the surface appears too smooth; or rather, it lacks the definite distinction of localization of separate threads, which is characteristic of cloth. The surface as a whole is not quite certainly localized, and appears to lie behind a medium which is brown but nearly transparent, and is of indefinite depth and uncertain localization." Br (6) "Sort of brownish haze, looking back onto surface. Surface hard to distinguish." Bi (6) "I felt as if I could make out a little of the fleckiness of the cloth. Hazy foreground of color, as if at the back limit of it there was a surface; rather definitely localized. Localization of the haze is uncertain. All this is somewhere behind the back screen." H (5) "A surface, adhering to an object, and then something

else in front, just as one might see through a vaporous film. The tridimensional seemed set off from the bidimensional objective. The surface was localized at about 25 cm. behind the screen." A (6) "It looks like a surface behind, and thick haze in front. The surface looks like soft cloth."

Group D. *Immersed Surface*

M (4) "A surface, or at least something opaque, behind a medium of decreasing transparency. The medium began at or close behind the screen, and extended to the unidentified object, which was rather doubtfully localized, somewhere between 10 and 20 cm. behind the back screen." M (2) "Very indefinitely objective. Close behind the screen, not beginning in any definite plane, was a vaporous brown color, very dense and very still, almost so dense as to suggest a viscous liquid. This vapor was nearly transparent where it began, but became progressively less so farther back. Behind it at an indefinite distance, not very far, however, was a gelatinous or perhaps solid something whose texture was completely obscured by the medium in front." A (6) "Something with a haze in front of it. Too rough to call it surface, *i. e.*, not smooth enough across." Br (2) "More haze than surface. It stops somewhere indefinite." Bi (5) "Haze seemed to end in a surface, but I didn't see the surface, either." H (4) "Solid and opaque behind, but with a gradual transition through cloudy to a thin, feathery gauze, and to a vaporous almost-transparency. Tridimensional with a hint of surface at the back. The haze obscures too much to allow objective reference."

It is clear that what we have phenomenologically before us, in this series of observations, is the gradual break-down of a visual object. We begin with the colored surface of a determinate thing, and we end with a sheer quality of color that is not the color of anything. Phenomenologically, we agree with Katz, the transition is continuous. The object of which the color is the surface becomes more and more vague, indefinite, unrecognizable, until only a vestige of objective surface, a hint of the surface of an indeterminate something, is left; and finally this last vestige of objectivity itself disappears.[17]

Psychologically, however, the picture is very different. At first we have the fairly simple perception of the surface of a material object;[18] this perception grows more complex, as the surface recedes and the haze before it becomes prominent; in the stage which we have distinguished as Group D the percep-

[17] It was probably one of these intermediates, and not the film proper, that was perceived by the patients of Gelb and Goldstein. See K. Goldstein und A. Gelb, Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter: IV. A. Gelb, Ueber den Wegfall der Wahrnehmung von Oberflächenfarben, *Zts. f. Psych.*, 84, 1920, 193-257.

[18] 'Surface' is such a visual space-phenomenon as it has been worth while to name, and therefore to make into a formal, text-book 'perception'. If any of the other phenomena here noticed corresponded regularly with our modes of apprehension of the outside world; if, that is to say, veiled, or fogged, or immersed surfaces were part of our everyday experience; then they, too, would have been listed as definite 'perceptions'. They are, however, so uncommon that they have not been identified or stabilised by name.

tion is at the height of its complexity: we have the vestige of a surface, covered by a haze which is denser in its remote portion and becomes clearer as it extends nearer to us; and in the next following stage, that of the film proper, we have (with the disappearance of the last remnant of surface-color) no perceptive object at all, but a sensory datum, ultimate and unanalysable. If we persist in taking up toward this datum the perceptive attitude which we have maintained throughout the series, we are obliged to characterize the 'object' by negatives: it is not now localizable, it is not dimensional, it is not objective. There is, then a continuous increase of psychological complexity up to (and including) Group D, and then there is a sudden break, from 'perception' to 'sensation,' from highly complex to absolutely simple.

If we may trust the reports of the *O*s, the determining factor throughout this experiment is *localization*, and the phenomenological continuity of the series depends upon the steadily increasing difficulty of localization as cleancut surface is left and film is approached. The following reports are typical.

Bi (2 G) "When I get my fixation on the cloth, it flattens right out. Localization is definite. It's hard to keep it that way. It will cloud right up and be a disembodied green." Bi (3 G) "The color fogged up as I ran my eyes back, but as soon as I got definite localization there was nothing but surface, bidimensional and definitely localized." Bi (0 G) "The filminess and localization vary with fixation." Bi (3) "That film is determined as much by fixation as by the screens. I've got more or less at the same exposure from the same screen." H (0 P) "That thing comes so near to being nowhere that it's almost nothing." A (0 P) "I believe if I could definitely localize how far away it was, I could get it as a surface, and the thing works both ways." Br (9) "I can get a surface or a film either one, depending on where I focus." Br (3) "Film at first. Then surface. These are changes in convergence, I'm sure." H (1 G) "I can fixate that so that it becomes just a cloth surface. My eyes water and ache. When I do fixate it, it is localized fairly definitely behind the second screen, 15 or 20 cm." Bi (5 G) "Fixation would slip and film would form. Then I would get fixation and film would disappear."

Summary: Effect of Size of Hole

In reporting the results of this Series I, we have given the size of the hole in connection with every introspection quoted. There was much variation (both from *O* to *O*, and with the same *O* at different times) so far as the exact size of hole is concerned at which the various modes of appearance were reported; there was, on the other hand, great constancy in the descriptions.

Films were reported most frequently by all *O*s with holes 0 and 1 (2 and 4 cm. diam.). H and Br occasionally reported film with holes of other sizes, up to 14 cm. diam. The film-reports with the larger holes came in the early stages of practice.

Surfaces were most frequently reported by all *O*s for the largest opening (20 cm. diam.). With the cloths of very open weave, especially the green and orange cheesecloths, surface was often reported with smaller holes.

The intermediates showed a good deal of overlapping. The medians for all Os were: soft surface, 18 cm.; veiled surface, 14 cm.; fogged surface, 10 cm.; immersed surface, 3 cm.

Series II. *Effect of Localization*

From the results of Series I it seemed probable that with our experimental arrangement the essential condition for the appearance of surface on the one hand or of film on the other is the localization or non-localization of the color quality. If this inference proves true, we have already answered half of our third question. We have found what changes the sheer color quality into the color of something. We should now be able, therefore, to change film colors to surface colors by giving the stimuli a definite localization. In Series II we attempted to test this hypothesis by bringing a strong localization-motive to bear upon what had been reported as a film color.

The colored stimuli used were Milton Bradley colored papers and the dark brown rep, because in the preceding series these had given pure film colors with larger holes than had any of the others. The most natural and obvious cue to localization was a fixation point in the center of the color-field viewed through the double screen.

We used both black and colored fixation points. They were cut with a bevelled circular punch and were slightly more than 1 cm. in diam. The usual exposure time in this series was 5 sec. Longer and shorter times were tried without affecting the results, except when prolonged fixation was accompanied by staring. The following instruction was given. "After the usual Ready Now signal, a colored disc and a differently colored fixation spot will be exposed. Be sure to maintain fixation at the fixation spot. Report on the character of the spot and of the colored field in terms of localization, dimensionality, texture, and objectivity."

At first the Os had great difficulty in fixating the point, because the edge of the back screen offered rival fixation points. When they had learned to maintain fixation upon the spot, the color was almost always seen as surface. This procedure was possible for all except the 2 cm. hole, which did not permit of binocular fixation of the fixation spot.

A (2 BL, o)[3] "Behind the back screen several inches. Small round piece of orange paper placed on top of a piece of blue paper." M (1 D, bk) "Cloth with a black spot on it. Very definitely localized. Texture and weave very plain." H (3 D, bk) "If I attempt to fixate the color beside the spot, it is possible to make the color a surface in a plane with the spot." H (2 BL, o) "When I first looked at that, I was sure it was simply one bit of paper on another. Definitely surface behind the screen, opaque and

[3] The large letters after the figure in parentheses indicate the background; the small letters, the color of the fixation point. The backgrounds used in experiments from which these introspections are quoted were [illegible] brown rep (D), and the following Milton Bradley colored papers: [illegible] green (BG), dark blue (BL), and red (R). The fixation points were [illegible]e (o), yellow (y) and black (bk). Many other combinations were of [illegible]e tried. The colors used did not affect the results at all.

solid." Br (2 R, bk) "A bit of red paper with the black stuck on it." Bi (1 R, bk) "I saw the spot pasted on in front. Both the purple and the spot were surface colors."

The spot and the color nearly always appeared as surface at first. Sometimes under steady fixation, even with the 5 sec. exposures, they took on a filmy appearance.

Br (2 R, y) "Yellow circle on a red background, which seemed at first to be surface; but as I looked, that seemed to be red air or space, not filmy enough for air. Momentarily it looked as if the yellow were in front of it, floating." H (2 BL, o) "Orange surface, bidimensional, definitely 15 cm. behind the screen. Definitely paper. The blue was simply some kind of blue surface. It might have been anything opaque and solid. As I stared at the orange spot, the blue became indefinitely localized, except that it dropped behind the orange."

In spite of these complications, we had shown conclusively that a visual cue to fixation was sufficient to change to surface an appearance which would otherwise have been filmy. This result in isolation would have been equivocal, however, because a bicolored film might perhaps be a visual impossibility. We therefore decided to see whether the cue to localization might not just as well come from some other sense department. The sense departments chosen for experiment were the tactual-auditory and the auditory.

In the lower part of each member of the double screen and also of the cardboard to which was pasted the colored stimulus were cut small holes 2.5 cm. square. A long rod passed freely through these holes. A piece of cardboard could be set up either against the color screen or in front of it in any desired position. *O* was allowed to feel through the screens with the rod until the tip of it touched the cardboard and then to tap, while looking at the colored field. Before the stimulus color was actually exposed, *E* put the pointer through the double screen in position for the tapping. Then the color was exposed for 5 sec., during which time *O* tapped the cardboard and looked at the color.

At first the *O*s found it impossible to connect the surface felt with the color seen.

A (2 BL, poking) "I don't think I'm realising very well that I'm poking the identical thing. That's a hard thing to do. I'm looking at this thing up there and poking this thing down here, and there's no connection between them." Br (1 BL, poking color screen) "I can't think of those two as being the same. It was surface I was touching back there, but it didn't look and feel the same." M (2 BG, poking color screen) "The object poked seems to have no relation to the color." H (2 BL, poking in front of color screen) "Curious feeling of the unrelatedness of the two things. The things simply don't belong together. I certainly was not poking at the color; for the most part behind it. The color is not at the screen. It is unrelated to the screen, but doesn't seem very far beyond it. One would be perfectly willing to grant that it *might* be twenty feet beyond. For the most part I was punching behind where the color lay." Bi (2 BL, poking) "I don't see any point to sticking this pointer down there. I see color up here in this position and away off in some other universe I stick this thing through. I see the color and the color is nearer than the pointer."

After about six hours' practice, however, the color, which had appeared filmy, could be transformed to surface by the tapping, though it was rarely possible to hold the surface throughout the period of exposure.

Bi (2 BL, poking) "Sometimes while I was localizing the tip of the pointer, I seemed to see a surface." Bi (2 BL, poking) "If I turn my attention away from the color and think where that pointer is, I can see the color as blue paper." Br (1 BL, poking color screen) "At first they seemed quite incompatible, and then the blue receded backwards while I poked and became for a moment surface lying some distance behind the hole, but I couldn't continue seeing it that way." Br (1 BG, poking) "At one of the pokes, the color receded and became a flat surface and the tip of the pointer was touching the same surface." H (2 Bl, poking color screen) "If I attend very closely to the sensations (both tactual and auditory) that come from the punching, then the thing does change. I'm not sure just what happens. The color does get thrown back there and fairly definitely localized, pretty definitely bidimensional. It certainly becomes more like an object, though just what I couldn't say; more like a smooth, painted surface, fairly opaque." H (1 Bl, poking in front of color screen) "If I definitely focus at some distant point, although I can't see the point,—but yet I have this tactual cue that helps me to establish and hold it,—then the color seems to retreat and become very suggestive of a surface." H (1 BL, poking in front of color screen) "I then attended to the punching and tried to fixate a plane perpendicular to the end of the stick. At first the color was indefinite in localization but, as fixation steadied, there was a sudden shift of that color. It seemed to start to retreat, and then became very suggestive of a surface perfectly stable in localization for a second or two." A (2 BL, poking and told to concentrate attention on the poking) "It seems every now and then that I just poke a flat surface, and it looks like a flat surface." A (2 BL, poking) "It's hard for me to think that I'm poking the same thing that I see, but when I do get that realized, all of a sudden, just in a flash like that, it's a surface behind the screen."

In this experiment the cues to localization were both tactual and auditory. The *O*s were guided not only by sensations in the hand and arm, but also by sounds produced when the rod struck the cardboard. It therefore seemed advisable to see whether sound alone was an adequate cue to the change of film to surface.

Immediately after the color had been exposed, *E* tapped without jar, either on the back of the cardboard to which the color was pasted, or else on another piece of cardboard of the same kind, held in the hand before or behind the colored stimulus. The taps were mostly given in pairs, with an interval of 0.5 sec. between the two taps of each pair and an interval of 2 sec. between pairs. The length of the exposure varied with the number of taps given. The usual number was 5 pairs, requiring an exposure time of 15 sec. The total exposure time seemed to make no difference to the general results.

At first, again, the *O*s found it impossible to link up the sound and the color. The two experiences seemed to be disparate and unrelated.

Br (1 BG, tap 2)[20] "All I can say is that I couldn't bring the two ideas together. The color is much nearer than the sound. The hollowness of the sound gave an idea of solidity, which was not borne out by the filminess of the color. The sound was from cardboard, distinctly objective." A (2 BL, tap 10, in front of color screen) "I can't realize that it's the same thing. Seems as if you're tapping behind the color. Your tapping doesn't affect the color. I'm looking at the color, and you're tapping away off, but they have no connection for me. I can't get them connected." Br (1 BG, tap 2) "No realization. I saw the color as nearer than the sound." Bi (1 R, tap 10) "The auditory localization is very complicated. I'm afraid I'm paying too much attention to auditory localization and not realizing that the sound comes from the colored stimulus." H (1 BG, tap 1) "No change. I never localized the tap so far as fixation was concerned." H (1 BG, tap 10, in front of color screen) "I didn't succeed in bringing the tapping and the color into relation." A (1 BG, tap 10) "The color is in the focus of attention all the time, and I hear the tapping as an incidental noise."

Again, however, when the *O*s had had five or six hours' practice in these auditory localizations, the colors were changed from film to surface as they had been by the other two methods.

H (2 BL, tap 10) "It's hard to fix the distance of those taps and hard to fixate the point with the eyes. However, there was some shift over to the surface thing, *i. e.*, it became definitely localized 20 cm. behind the back screen, and definitely bidimensional." H (1 BL, tap 10) "It shifted to surface with the first pair of taps. That was maintained till between the second and third pair. Then I went over into a stare, and the thing became pretty positively filmy. After the third tap, I shifted back to fixation of the place where the taps came from and it became definitely surface with lights and shades,[21] and nearly papery." H (1 BL, tap 10) "I tried to fixate a point just behind the back screen, but this side of where the tapping took place. I maintained that till after the second pair of taps. It seemed to throw the color back a little, but otherwise it remained pretty good film. Then I fixated the point of tapping and got the surface thing." H (1 BG, tap 10, behind color screen) "The color tends to fluctuate between taps. The first pair of taps made no difference. With the second pair, the color definitely retreated. All of a sudden you find it back. You didn't see it go back. Bidimensional. About 20 cm. behind second screen. Certainly a surface, opaque and solid...... The transformation to surface always came with (or just following) the pair of taps." H (2 BL, tap 2) "After the second tap came a readjustment of fixation, and it became definitely surface." H (1 BG, tap 1) "It certainly does shift over to surface with steady fixation, when the eyes are set to fixate a point where the sound is localized. The shift came just after the tap, and then I tried to localize it in terms of the screen, but as soon as I tried to see where it was relative to the screen, it became cloud. Then when I returned to the fixation of the point tapped, it became surface again." Br (1 BG, tap 2) "Yes, they seemed to come from the same surface that time." A (1 BG, tap 10) "It seems that after I get fixed for realizing that back screen and I hear you strike it, I'm looking at the color and I'm conscious that it's there, but the

[20] The figure after the word "tap" indicates the number of times that *E* tapped during the exposure preceding the report quoted; thus "tap 2" means that a single pair of taps was given.

[21] There were of course no lights and shades; yet the appearance of surface occasionally led to their report. So one *O* reported the weave of a cloth, when in fact he was looking at a disc of colored paper. He had, it is right to say, been observing cloths during several preceding experiments; and the phenomenon was evidently an ordinary tied image.

tapping is in the focus of attention. But then, after you've stopped tapping, the color gets into the focus of attention again. In that shift I see the color as back behind the back screen, in two dimensions, just a flash of it. Then it goes back to what it was before." A (1 R, tap 10) "I got a flash of it receding from the back screen and being a perfectly flat surface right after you tapped." Bi (2 BL, tap 10, in front of the color screen) "Once I saw a surface when you tapped. Then again I didn't. Then you tapped again, and I did see a surface. Between taps the color was filmy." Bi (1 BG, tap 10) "For the most part the color was in front of the tapping. Just once I seemed to get a fair surface. I think it came partly by way of fixation." M (2 BL, tap 10) "The color changes from film to surface with each pair of taps, but returns to film between pairs."

Thus by giving localization through three different sense departments we had changed to surface a color that would otherwise have been filmy. In all these experiments, therefore, we are referred, as we were in Series I., to the predominating influence of localization. We have been able to change a film color into the surface color of a determinate object by introducing motives to localization, and the result has been in principle the same, whether the motive was visual, auditory-tactual, or auditory.

Series III. Observations of Familiar Objects

Series III was designed to answer the second question proposed in our introduction: "Can it be shown experimentally that the object-consciousness is sufficient of itself to ensure the perception of surface color?" If this were so, the knowledge that one is really looking at a familiar object should enable—nay, even compel—one to see surface, and prevent the reduction of surface to film.

Two forms of double screen were used, one of which was identical in essential details with that previously described, except that a single-sized opening of 9 cm. diam. was used. *O* sat at distances of 1.5, 1, and 0.5 m. from the front screen. No head rest was used. *O* could move the head and eyes freely. No instructions were given with regard to fixation. Most of the *Os* found it easiest to fixate on the edge of the hole in the back screen.[22]

The stimuli were familiar objects: a board, a fur neck-piece, a strip of felt, a satin cushion, a woolen sweater, aprons, coats, and cloaks. *E* placed the stimulus in such a position that it entirely filled the hole, and also projected above the upper edge of the double screen. In direct vision *O* could not have seen the stimulus above the screen and in the hole at the same time; but by slight movements of the head and eyes it was possible to make swift comparisons of the two. Under these conditions, the *Os* never described the color in the hole as ordinary surface color, except when they also reported some detail of structure which offered a fixation point.

[22]Katz, *op. cit.*, 38.

The other form of double screen was used only in this series. It was designed to compel parallel vision.

Like the regular form, it was made of grey cardboard fastened to wooden frames. In each screen were cut two holes of 1.5 cm. diam. The wooden frames carrying the cardboard screens were then clamped at the front of a table, and *O* sat as close as possible and looked directly into the front screen, as into a binocular telescope. Since the interocular distance differed slightly for different *Os*, different double screens constructed on the same principle were used, and the precise distances between the holes were chosen empirically to suit the *Os*.

The stimuli were small familiar objects: soap, chocolate, paper, and the like. The stimulus was first handed to *O*, and he was allowed to examine it as he pleased. Then *E* placed it behind the double screen. *O* did not know how far from the double screen the object was to be placed. With this exception, the *Os* worked with full knowledge of the apparatus and manipulations. They therefore had "the consciousness of an object upon which the colors arise," which Katz regards as the essential condition for perceiving surface. Yet, under the above conditions of experimentation, surface colors were never reported.

III. Bulk to Film

Series IV. Episcotister Before Surface With Fixation Point

We planned Series IV as a direct parallel to Series I. We intended, that is, to begin with a positively bulky color, and to carry this over by intermediates, phenomenological or psychological as the case might be, to film color. Bulky colors were secured without the use of actually bulky stimuli by means of colored episcotisters.[23] When an object is seen through a rapidly rotating episcotister, it appears as if seen through a haze of color. Under certain conditions, this haze expands to a positive bulk.

The episcotister used in these experiments was made in two identical parts, each of which was constructed from a disc of thin but very stiff cardboard, 51 cm. in diam., by cutting out 6 sectors of 30° each, leaving 6 equally spaced sectors of 30° each. These were covered on the one side with blue paper and on the other with reddish orange paper. These two colors were chosen for convenience. Any other colors might have been used. In cutting out the sectors a cardboard margin, 2 cm. wide, was left on the outer edge, and a cardboard disc, 12.5 cm. in diam. at the center for support. The episcotister was mounted on an electric motor, operated by pedal. By proper combination of the two pieces, it was easy to use any amount of color from 180°, which gave a slight, vague, barely perceptible haze in front of the background, to 360°, which of course was no longer an episcotister, but a solid color disk. It was possible to use either pure blue or pure red-orange or any desired mixture of the two colors, by using the blue side of the one disc and the red-orange side of the other. Other differences in color could be obtained by varying the color of the background. The backgrounds used were whole sheets of Milton Bradley colored papers tacked to the wall of the dark room. The table, carrying the double-screen and the episcotister, was so placed that the back screen was 65 cm. from

[23] *Ibid.*, 310.

the colored background. The episcotister could be moved to any desired position between the screen and the background. The exact position seemed, so far as our results were concerned, to make no difference. Distances actually used were 10, 15, 20, 25, 30, 35, 40, 45, and 50 cm. from the back screen. Even with the two extremes, no consistent difference in reports could be discovered. As one *O* remarked: "I don't seem to get any variation in the experience, however you move things behind." In the latter part of the series, the distance between the episcotister and the back screen was kept at 20 cm.

The *O*s were given typewritten instructions, similar to those used in preceding series. The exposure time was 5 sec.

Under these conditions, with screen 3 (8 cm. hole) appearances precisely similar to those of Groups B, C, D of Series I were secured,[24] an increase in the amount of color in the episcotister having precisely the same effect as the substitution of smaller holes had had in the series without fixation points. A further increase in color, however, did not result in the immediate appearance of film, as it should have done on the analogy of Series I. Two other stages, which we have designated as e and f respectively, were found. In Group e, the last vestige of surface is gone, and the fixation point, whose contours are by this time badly blurred, is seen floating deep in a thick substantial mist.

Bi (3 O, bk, 336 O)[25] "The film is well this side of the fixation point and definitely behind the screen. It hints at dimensionality. The surface has been out for quite a while. I didn't realize it was gone." Bi (3 O, bk, 330 O) "Now the fixation point is away back in a dense tridimensional orange. It might be daubed on the back of it for all I could tell, only the film wouldn't support an object." H (3 O, bk, 240 O) "Uncertain localization. Curiously, the spot seems to be behind the screen. The color might be this side of the spot. The color seemed more indefinite than the spot. Very fuzzy, felty, soft. No particular objective reference. Cloudy. Translucency is suggested." H (3 O, bk, 330 O) "The spot always seems to be back in the color, like something indistinctly seen through haze. The haze is simply out there, and that is all you can say. The haze is around the spot and in front at the same time." Br (3 O, bk, 330 O) "Fairly dense haze. The fixation point is floating somewhere in the haze. The position of the whole thing seems moved toward the hole, but not as far back as when seen on the surface." Br (3 O, bk, 336 O) "The haze was still denser. The fixation point came and went. Very indistinct. It might be located anywhere within the haze." Br (3 BL, bk, 270 BL) "The dense haze fills all the space up to the hole. The spot is somewhere behind that blue haze or immersed in it, so that you can see the blue in front of it." A (3 BL, bk, 342 BL) "Dark blue mist. I could see the fixation point through it a good way back. I couldn't see any surface." A (3 O, bk, 339 O) "I looked into an orange mist that came up to the back screen, and far back in the mist I could see the fixation point very vaguely. I don't think the mist looked as if it went farther back than the fixation point, but I'm not certain." A (3 O, bk, 348 O) "Looking into orange mist. It came up to the

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[24] For facility in cross comparison, we shall continue to refer to these by the same letters as in Series I, but shall use small letters instead of capitals, so that the two series may be kept distinct.

[25] The second number in the parentheses indicates the number of degrees of color in the episcotister; the letter following it, the color used on the episcotister.

back screen, and I don't know how far it extended; not very far. Fiery kind of mist. It looked as if it might have been made up of thousands of little particles. I could make out the fixation point, somewhere in the mist. Its outline wasn't distinct." M (3 BL, bk, 210 O) "Fixation point barely visible through a dull pink tridimensional haze. The fixation point is indefinitely objective and bidimensional."

In Group f, the fixation point is no longer an object, localized with difficulty in the midst of a thick haze. It has become a film, as positively indefinite in localization as the color around it, which approximates the films obtained by the reduction of surfaces, the only difference being that it is slightly more suggestive of tridimensionality.

H (3 BL, bk, 180 BL) "At first glance it's the old indefinite cloudy thing. Then I got my fixation point, but couldn't hold it. There was a good deal of fluctuation. Localization was not very certain, but there was a tendency to put it behind the back screen. Bidimensional, but might easily have been tridimensional. No objective reference. Strong hint of translucency, but yet I couldn't actually say that I saw into it; more of the cloudlike effect. The fixation point shared the character of the color." H (3 BL, bk, 300 BL) "I saw the fixation point dimly all the time. No difference between the spot and the blue. Indefinite localization. Strong hint of tridimensionality. Strong hint of translucency, and yet I couldn't see into it. For the most part I haven't been able to focus on the fixation point." Br (3 O, bk, 330 O) "I saw the fixation point as a very faint grey filmy spot, not flat. It had a film in front." Br (3 O, bk, 342 O) "Film, perhaps a little denser. I see the fixation point as a grey nebulous mass in the center." Br (3 O, bk, 348 O) "Very dense film. Slight greyishness in the center. It didn't have any definite outlines, and didn't appear like a fixation point at all. Just a bit of grey film." A (3 O, bk, 339 O) "I could see the fixation point very vaguely. I don't think the mist went back any farther than the fixation point, but I'm not certain. I can't say whether the fixation point is standing out in the mist or whether it's behind it." M (3 BL, bk, 180 O & 150 BL) "The fixation point is barely visible in a mass of red fog, which is tridimensional. The fixation point is filmy, indefinitely localized and non-objective." Bi (6 BL, bk, 324 BL) "Fixation point is back again just a little. I can make it out with some certainty now, just a little dark spot. It's filmy, too." Bi (6 BL, bk, 327 BL) "Able to see the fixation point about a third of the time. It's also filmy." Bi (3 BL, bk, 180 O & 30 BL) "Fixation point and fixation screen are both films now, but not very far removed from surface. All the color is close together in a thick plane, no texture."

When the amount of color in the episcotister was increased so much that the fixation point was no longer visible, the resulting appearance was a film, practically identical with the films found in Series I. One *O* called attention to this fact.

Bi (6 BL, bk, 330 BL) "Fixation point gone. It's a great film, filling up the whole area behind the cardboard screen. As far as I can tell it doesn't look any different from the plane colors of the other series."

Here, as in Series I, there were of course a few equivocal cases, which seemed to fall midway between two of our groups. The following may serve as examples of a suggested intermediate between e and f.

Br (3 O, bk, 180 BL & 150 O) "Pink film. The fixation point floats in the film. Fairly dense. Pinkish right up to the hole. The spot is filmy, but seems to lie right back in the film." H (3 BL, bk, 180 BL) "The fixa-

tion point was definitely localized at 30 cm. or more behind the screen, probably bidimensional. No objective reference. Opaque, solid. The color was different. I know little about the color. Not definitely localized except behind screen. So far as I can say, it was bidimensional. I don't know that I saw it dimensionally at all. No objective reference. Opaque; whether solid or not I don't know."

The dependence of all these appearances upon fixation is illustrated by many reports, of which the following may serve as examples.

A (3 BL, bk, 306 O) "The fixation point is back. I can't see it on a surface. When I see the fixation point, the mist extends lots farther back than when I don't see it." A (3 BL, bk, 312 O, immediately following the previous report) "At first I looked into orange mist, which had a little tinge of lavender, extending back an inch or so. Then I looked carefully and saw the fixation point, and the mist spread away back." Br (3 BL, bk, 300 BL) "No fixation point visible, just dense blue film. I can't see through it as far as when I looked through it to the fixation point." Br (3 BL, bk, 321 BL) "When my fixation comes front, the color becomes very dense and occupies all the space between my fixation and the screen; when I maintain fixation, I get what I described last time, *i. e.*, approximate surface behind at the fixation point, with dilute film out in front, which came toward the screen, but not to it. The surface was bidimensional; the rest, tridimensional." Bi (3 BL, bk, 324 BL) "No fixation point visible. I could make out an approximate surface back where the fixation point would have been if I could have seen it. The rest was thick haze, uncertainly localized." H (3 O, bk, 336 O) "If I look simply at the color I get it indefinitely localized, bidimensional, hinting at tridimensionality; sort of a rudimentary tridimensionality. No objective reference. Pretty much the cloud. A little hard, maybe. Hints at translucency. The spot is simpler a darker bit in the color. If I look for the spot, then I seem to be able to throw the spot back."

A short supplementary series was tried in which the background, instead of being a plain sheet of colored paper, was a sheet of cardboard bearing the printed symbols PSYCHOLOGY I B and a rough drawing of an arrow. Screen opening no. 3 was used, and the amount of color in the episcotister was varied from 180° to 330° by steps of 30°. The resulting appearances were surfaces obscured by hazy or bulky colors, which varied in depth and density according to the amount of color in the episcotister. The more color in the episcotister, the denser of course was the haze. The apparent depth or degree of tridimensionality increased up to about 270° and then diminished as more and more color was added. When the letters could no longer be seen the resulting appearance was the familiar film.

Series V. Episcotister Before Surface Without Fixation Point

Series V was undertaken as a forlorn hope to see whether our practised *Os* might possibly report the impression of bulky color, as in stage e of the preceding series, even though the fixation point had been removed. The arrangement, therefore, was identical with that of Series IV except that the colored backgrounds were uniform over their entire surface.

The attempt was partially successful. Usually, it is true, the reported color was almost identical with the films obtained from the reduction of surface colors; like other films, it was non-objective, unlocalized, and pretextural; but it often seemed to hint more strongly at tridimensionality; and sometimes tridimensionality of uncertain amount was positively ascribed to it.

Br (3 O, 180 O) "Red bulk rather than red film, like a red liquid." Br (3 O, 180 O) "Dense red film, redder than before. It's almost as if I were looking through a red light or into a brilliant flame. Luminosity." H (6 O, 180 O) "Too luminous for cloudy; too much like a glowing ember where you only see the glow. No other perceptive motive there. It tends toward a glassy or liquid bulk, though it isn't that." Bi (3 O, 270 O) "Thicker now and more nearly tridimensional than before." A (3 BL, 180 BL) "It comes up to the back screen and seems to come out just a little bit at the middle; just floats out like mist." H (3 O, 180 O) "Suggestion of tridimensionality. Hint of depth. Sometimes part of the field was almost bulky." H (3 O, 279 BL) "For the most part behind the screen, but not very definitely localized. Tridimensional for the most part. Translucent, like bulky liquid tending slightly toward gaseousness. It definitely had depth." M (3 O, 180 O) "Very dense haze. Comes up to the back screen and extends back a short distance." M (3 BL, 330 BL) "Stationary blue haze, doubtfully tridimensional, suggestion of translucency. Perhaps lighter in front and dark behind. Indefinitely localized."

Associated with the tendency toward bulkiness, the *Os* sometimes reported a sort of incomplete objectivity.

H (3 O, 180 BL) "For the most part tridimensional, although the tridimensionality increased and decreased, with a wavelike effect. Objective reference, but not specific. It didn't come as an object, but you could make it into an objective something."

Thus in Series V we seem to have found something like true psychological intermediates between film and bulk. Equivocal appearances tending more or less toward tridimensionality were reported from time to time throughout the series. Whereas, under conditions of binocular vision, film changes into surface at a single step by way of superadded localization, film seems to pass into bulk by insensible degrees, as if by mere extension of that suggested but unrealized tridimensionality which all *Os*, those of Katz as well as our own, have remarked.[26] We believe, nevertheless, that *these intermediates are still nothing more than phenomenological*. It must be remembered that the bulk in our bulk-to-film series was itself only generically objective, an affair of haze or mist in general, and not specifically objective like the surface color of paper or cloth; nor was the background so marked or shaped as to suggest a determinate object seen through an intervening haze; it was mere expanse of color with a central fixation point. Even in stage e, *O* may report entire lack of objective reference, especially at an early stage of training. Hence it is probable that we are here in presence of perceptive formations whose likeness to any common object of percep-

[26]Katz, *op. cit.*, 7.

tion is markedly less than that in the formations named b, c, d. This conclusion is borne out by reports of vague or general objectivity, reports which increase in frequency as the training of the *Os* progresses.

For the reader's convenience we insert here a brief table of the phenomenological results of our two main Series.

SERIES I		SERIES IV
A	Soft surface	
B	Veiled surface	b
C	Fogged surface	c
D	Immersed surface	d
	Substantial haze	e
	Cloudy haze	f
	FILM	

A, B = b, C = c, D = d, and e are without doubt perceptive formations. We believe, as has been said above, that f is also psychologically a perception; in spite of the unfamiliarity of colored hazes in everyday life, the *Os* tended more and more, as observations went on, to report a vaguely objective reference. The fact, however, that any degree of doubt or hesitation can arise at this point supports our conclusion that the ultimate film is more suggestive of tridimensionality, less positively bidimensional, than Katz has made it.

In connection with these bulky hazes, a rather curious phenomenon seems worthy of remark. Some of the *Os*, most frequently Br, reported not only positive tridimensionality, but also differences of hue, between the front and back regions of the haze.[27]

Br (6 R, 300 BL) "Dense blue film with the faintest tint of purplishness in the front and dense blue toward the back." Br (6 R, 270 BL) "Deep purple, pinkish toward the front...... In all these the film begins at the screen and goes back from there. Usually the front is filled in with a lighter film, misty." Br (3 YG, 180 BL) "Greenish film, whitish-yellowish at the front and green farther back. It starts at the back of the back screen and goes back a long distance, almost to infinite distance. It's the sort of haze that I've seen heaps and heaps of times at sea."

One of the byproducts of Series IV and V was the discovery that bulky films could be produced with somewhat larger holes than the films of Series I. The difference made by the use of the episcotister is well illustrated by the following pair of reports, taken successively in the same observation hour. The first was taken without episcotister in front of color, but with motor running, so that there was no auditory cue to the difference. The second was the same as the first, except than an episcotister with 270° of orange was between the color and the back screen.

[27] It will be remembered that the *Os* were not instructed to report upon hue. Hence these observations were spontaneous. It is possible that the other *Os* might occasionally have noted these differences, if the instructions had called for them.

H (6 O) "Quite definitely localized, bidimensional, fairly definite objective reference, in so far as it was a smooth surface (paper or anything of the kind). Solid, opaque." H (6 O, 270 O) "Very indefinitely localized. I don't know whether I can even say it was back behind the back screen. Bidimensional, I guess. There were moments when it seemed almost tridimensional. Hint of translucency. The term 'cloudy' is not adequate, 'filmy' is not adequate. It's just a bit of luminous color."

Series VI. Control Series

The best guarantee of the validity of experimental results is agreement among a number of trained *Os*. Our quotations have shown that there was in fact a very high degree of agreement. Series VI was planned as a further test of reliability. To see whether the use of the episcotister carried with it any imaginal suggestion which might have exaggerated the differences between the film colors of Series I and the bulky colors of Series IV, we turned the episcotister in such a way that it did not come in front of the colored background at all. The episcotister was left on the motor, so that it might run at the same speed and produce the same sound. The shadow of the episcotister fell on the wall some distance from the colored paper, so as to have no effect on its appearance. Since the colored backgrounds were tacked to the wall, they could not be jarred or in any way affected by the motor on the table. Hence it made no objective difference in the color whether the motor was running or not. The only difference between exposures during which the motor was running and those for which it was turned off was the sound. It was assumed that the sound might act as a suggestion to the *Os* that they were seeing the same sort of thing as in Series IV and V. Between exposures *E* moved the motor about so as to suggest, by sound again, that the episcotister was being put in front of the color or removed. No verbal suggestion was given.

The screen openings used were 3 and 0 (8 and 2 cm. respectively): screen 3 because it was that most frequently used in the series to be tested, and screen 0 because it had invariably given film in Series I. With *Os* A and Br screen 6 (14 cm.) was also used for purposes of comparison.

In every case the *Os* immediately or almost immediately discovered that the two experiences were identical. Br and A were absolutely unaffected by the suggestion. H, as was shown by the cautious and hesitating nature of his reports, was apparently affected by the suggestion in the first two observations, but in the third he settled down to objective report, and thereafter found no difference between the appearance of the color when the motor was running and when it was not. Fifteen subsequent trials failed to show any the least trace of suggestion. Similarly, Bi showed a possible slight effect of the suggestion in the first three trials; but even here the differences reported were scarcely

greater than those occurring in the course of a normal series; and his reports in 15 subsequent trials were entirely free from evidences of suggestion. This record affords objective proof of the reliability of the *Os*.

IV. The Monocular Film

Series VII. Holes of Various Sizes

In this series the apparatus and procedure were identical with those of Series I, except that, instead of making all observations binocularly, the *Os* were asked to alternate binocular and monocular observations and to compare the two.[28] The reports of all *Os* showed that with monocular observations size of hole makes less difference than with binocular. In every case the color appears as an almost bidimensional film, which tends to lie in the same plane as the back screen. In having less suggestion of tridimensionality about it than the binocular films of previous series, these monocular films are closer to the typical film as Katz conceives it. The following comparative observations illustrate this point.

H (°, l) [29] "With one eye the two holes seem very much in the same plane...... The color was just color. Non-objective and unlocalized. It was certainly behind the cardboards, but I couldn't tell more than that. It tended to come up close against the card. Dull, opaque, fuzzy, soft. Slightly suggestive of translucency; not much. Neither bidimensional nor tridimensional. Bidimensional spread is clear enough, but there is nothing definite about the third dimension. Tridimensionality is suggested rather in the softness, the fuzziness, the sponginess of the thing than in anything else." H (°, r) "Little difference between the two eyes. The texture was perhaps a little less fuzzy and soft, but still suggested the third dimension." H (°, b) "Funny how that changes! With two eyes, there is a definite space between the screens. The second was definitely 20 cm. behind the first. The color was still non-objective, localized at or behind the second screen. More certainly tridimensional, though not in any determinable amount. The texture was more translucent, more motile, as though there were liveness in the color. I think that's probably what we have called luminosity. More dense, glass-like; not clear, transparent glass, but a little more solid." Bi (°, r) "Equally filmy, but pretty much up to the hole. I tried to force my eye back in it, and it didn't work somehow. It left me with something that was not deep in the third dimension, but not surface." Bi (°, l) "I have a feeling that I ought to be able to force that film to be deep, but I don't believe I can. I can pierce it a bit, but it stays pretty much a flat, dense film at the hole." M (°, b) "Indefinitely localized, except somewhere behind the back screen. Non-objective. Soft, loose, penetrable texture. Dimensionality uncertain, but tridimensionality is at least strongly suggested." M (°, r) "Non-objective, just a patch of color in the hole. Soft, thin, filmy. Probably bidimensional, though here (*i. e.*, in monocular observations), where differences of localization are not

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[28] We took our cue for this monocular series from Katz' suggestion that monocular observation of ordinary objects affords a mode of appearance intermediate between film and surface: *op. cit.*, 9.

[29] Small letters after the comma in the parentheses indicate the mode of observation. Thus l means "with the left eye;" r, "with the right eye;" and b, "with both eyes."

so noticeable, dimensionality also seems equivocal. All things seem shoved up more nearly into the same plane. The front screen, the back screen, and the color all lie, or at least look as if they might lie, in the same plane." Br (2, b) "Soft, filmy brown medium. Localized near the hole and yet it seems to go back a bit from that. Dense, cloudy appearance." Br (2, r) "Same, except that it's like a plane, even, fine-textured cloth at the hole; really a film, but I can imagine a cloth like that; one of those soft georgettes." Bi (2, b) "I have a suspicion that there's a deeper film with two eyes than I saw with a single eye. The one with both eyes is like a fog; the one with one eye is like a rotating disc." Bi (2, r) "I looked for the deep character that I thought I saw before. It is not there with the single eye, though what I saw was certainly not a surface. It still had a filmy character." A (2, b) "I see a brown misty thing up in the hole and behind it, too." A (2, r) "I can't say whether it looked up at the screen or behind, but it wasn't misty-looking as it was when I saw it with two eyes. More surfacey. Not a slick surface. More like blotting paper."

Thus it is evident that monocular observations with the small holes gave a flatter, more nearly bidimensional color than did the binocular observations.

With the larger holes, the reverse proved true. Although the object could now be easily identified, it did not appear to have so sharply defined a surface as in the binocular observations.

M (9, b) "Ordinary cloth, definitely localized behind the screen." M (9, r) "Positively cloth, but greatly softened, and not a sharply defined surface. Velvety. Not much space between fore and back screens or between back screen and cloth. All are in nearly the same plane. Less certainty of localization than when viewed binocularly; or rather, less positiveness of difference in localization. The very question of localization seems out of place in these monocular observations." H (9, r) "Little difference monocularly between this and the last (hole no. 2, 6 cm.). Opaque, softened, a little more certainly behind the screen when I use the side of the hole as a reference point, but not when I look straight at it." H (9, b) "Fairly sharp surface. No hint of softness. Certainly a colored something. Cloth, or it might be a colored wall. Localized very definitely behind the second screen at a distance of 20 cm., more rather than less. No suggestion of depth, just an opaque, solid substance." Bi (8, b) "I get a film that goes all the way back to the background, but I can nearly see the cloth, if it is cloth. The cloth was away back by the wall. The haze was everywhere between the cloth and the hole. Part of the time it seemed equally dense throughout, and part of the time it seemed thinner up in front and denser behind." Bi (8, r) "Dense film. Thick plane at the screen. It huddles up to the front. It stays up. I can't force my gaze back through it." Br (8, b) "Surface, some distance behind second hole. Flat piece of cloth." Br (8, r) "Film, which is difficult to distinguish as film, because it seems that flat plane, lying on the back hole." A (9, b) "Back behind screen, 8 or 9 in., and now it looks with two eyes very much as it did with one a while ago, although it has more quickness and liveness." A (9, l) "Right up in the screen, and a little duller than before. By dull I mean that it hasn't that quick, lively look."

From these and similar observations it is evident that, with monocular observations, the largest holes like the smallest gave an appearance of filmy texture. In fact one *O* (H) remarked: "The one-eye appearance is nearly uniform for the whole range of sizes."

For the sake of completeness we append a few typical observations taken with intermediate sizes of holes.

Br (4, b) "More filmy and behind the back hole. I couldn't say whether it was really bulky. Indefinitely localized. No definite front surface." Br (4, l) "It looked a little more flat and seemed more a plane, but not as definitely a plane as when I saw it binocularly through those large holes." Br (4, r) "Same. Filmy, soft, fuzzy appearance, but not so thick as when I saw it with both eyes." Br (6, b) "Filmy, thick, dense, foggy." Br (6, r) "Flat, even, filmy thing; really like a spectral color hung in space." Br (6, l) "Same. More plane and even than with both eyes." M (4, b) "Brown surface, seen dimly and localized with difficulty behind a brown medium of increasing density which lies somewhere behind the back screen. The surface is cloth." M (4, r) "Brown cloth, but very soft, thin, and filmy. The brown might be at, or even slightly in front of, the screen. Certainly there is not much if any difference of distance between the screen and the color, or between the two screens; but the screens themselves are not localized with any degree of precision. Not so much suggestion of tridimensionality as with two eyes, but the surface character is not positive either."

Without stopping to multiply illustrations we may therefore say that, throughout our whole range of holes, including those that were large enough to permit a recognition of the object, monocular observations gave appearances which in texture, dimensionality, and indefinite localization approximated fairly closely to the Katz film.

Series VIII. Monocular Observations with Episcotister

Obviously the next step was to find out what happens to bulky colors under monocular observation. For this purpose we used an experimental arrangement identical with that of Series IV. In the episcotister we used dark blue, orange, and various combinations of the two. The background was a sheet of dark blue paper with a black fixation spot in the middle. As in Series VII, monocular and binocular observations were alternated for purposes of comparison. It was found that, whereas binocular observations with a suitable amount of color in the episcotister gave good bulky color, monocular observation reduced this to a filmy appearance.

H (3 BL, bk, 180 BL, b) "The spot was definitely 25 or 30 cm. behind the back screen. It tended to give considerable depth to the color. The color was transparent, dense, glassy, flinty. It was still non-objective. I don't know whether the color filled all the space between the screen and the dot, but it tended to have that meaning." H (3 BL, bk, 180 BL, r) "More nearly flat in form. Non-objective, but it suggested some kind of soft surface. Quite opaque. No translucency or transparency. The color was localized back at the dot. I don't think the color or the dot either was quite as definitely localized as the dot was before (*i. e.*, binocularly), but the color was with the dot." H (3 BL, bk, 180 O, b) "The fixation point was definitely 25 or 30 cm. behind the back screen. The color itself was certainly non-objective and pretty definitely tridimensional; a mass, almost a body of it, extending from the region of the screen back to the dot. I couldn't be sure whether it extended clear to the dot or not. Translucent, soft, not much glassy." H (3 BL, bk, 180 O, r) "The color and dot

seemed to lie together and tended to be localized nearer the screen, but there was not absolute definiteness of localization. Less massiveness to the whole experience, but yet not a definite surface with objective reference. Soft, fuzzy, suggestive of tridimensionality but not in the same sense. The whole thing is dead, static, more like a soft surface. No suggestion of translucency or transparency." A (3 BL, bk, 270 BL, b) "I saw tridimensionality that time. Thick fog came up to the hole. I could see into it. I couldn't tell how far it extended. The fixation point was in it somewhere." A (BL, bk, 270 BL, r) "Bidimensional, as if I were looking at something that let the light through; but I saw it in two dimensions. Like thick colored glass that's soft-looking, but you don't see anything but the front of that glass; like glass in church windows. If you didn't know it was glass you'd hardly call it glass. Not transparent, but translucent." A (3 BL, bk, 270 BL, b) "I couldn't help seeing it as tridimensional. I tried to hold it in the same place as with one eye, but I couldn't. The two things don't look anything alike." Br (3 BL, bk, 210 BL, b) "Bulky, dense medium. The fixation point was buried in it somewhere." Br (3 BL, bk, 210 BL, r) "Flatness, filminess, rather than surface, and yet it's all in one plane, or almost all in one plane. With two eyes you seem to be looking into something almost glassy-like. This is like the blue of the sky. This has the appearance of being penetrable, but you don't have that impression of seeing space that you have with the other." Bi (3 BL, bk, 180 BL, b) "Hazy smokiness which goes clear back to the fixation point. It's open and loose. I didn't see it come clear up to the screen I didn't see it fall short either." Bi (3 BL, bk, 180 BL, l) "This tended with one eye to be flat. The fixation point tended to come up to the screen. Anyway they got crowded together." M (3 BL, bk, 270 BL, b) "Fixation point badly blurred and localized with difficulty in a dense, semi-transparent fog, which began at or near the back screen and went back an uncertain distance." M (3 BL, bk, 270 BL, r) "Color and fixation point are both filmy, and seem to lie in approximately the same plane as the screens. Both screens seem to lie in very nearly the same plane. No part of the total experience is localized with any degree of precision. The color appears soft but not actually penetrable. It is non-objective."

Hence it is evident that all the color appearances which we have here studied, when viewed monocularly, take on a more or less filmy appearance. They become indefinitely localized, pretextural, predimensional; and except where familiar configuration carries with it the meaning of a familiar object, as in the case of the screens themselves or the brown cloth as seen through the largest holes, they are also non-objective.

V. Conclusions

1. Our experimental results indicate that the simple visual datum is a "film," predimensional but tending more strongly toward tridimensionality than Katz' bidimensional *Fläche*. The film is also pretextural, non-localizable, and non-objective. It has an attributive character of spatial spread or diffusion, to particularize which we must have recourse to qualified perceptive analogy.

2. There are no psychological intermediates between film and surface. Our results show a sharp break between the true film and the immersed surface, the haziest of the surfaces, a

break which involves a complete change of attitude. The localization of the color in the case of the surface is the determining factor in the shift from film to surface.

3. Between film and bulky color the shift of attitude is more tentative and cautious, and the transition is in a sense more gradual. There are, nevertheless, no true psychological intermediates.

4. There is a wealth of phenomenological intermediates, any one of which might have been stabilized as a perception if our everyday experience had required such stabilization.[30]

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[30]We make no claim to have reviewed the whole series of intermediates. It is possible that another approach to the general problem, and in particular the bringing to bear upon the film of strong perceptive motives other than that of localization, might lead to another classification and might reveal new phenomenological modes. We can say, however, that in a good deal of casual experimenting, such as naturally suggested itself in the course of an investigation like the present, we have found no 'leads' toward a further variety of visual appearance at the level of our enquiry.

CAN THE PSYCHOPHYSICAL EXPERIMENT RECONCILE INTROSPECTIONISTS AND OBJECTIVISTS?

By J. R. KANTOR, University of Indiana

In the following article the writer assumes that it is exceedingly desirable (in order to establish psychology as a science and to promote necessary mutual understanding) that psychologists should agree upon the fundamental data of their investigative domain. Accordingly, he undertakes to point out that such an agreement not only appears possible but that there is in fact more agreement between psychologists of various schools than really appears on the surface. In fact the writer believes that he has found a possible basis for an almost complete agreement between introspectionists and objectivists in some recent discussion of the stimulus-error and the psychophysical experiment.[1] In addition the writer attempts to characterize what to him appear to be the conditions underlying the present dissension among psychologists and to propose some suggestions for overcoming such disharmony.

I. Why Agreement among Psychologists seems Necessary. Are not psychologists after all interested in the same series of natural events? If this question is affirmatively answered then agreement there must always be between psychologists no matter what their presuppositions are, provided that they exercise meticulous care to keep close to the facts within their special province of investigation.

Can it be to the advantage of psychology if the several psychologists indulge in such violent disagreements that they can question the value of each other's work and its scientific validity? And yet to the existence of such derogatory ideas of each other's work on the part of psychologists the content of our psychological literature could hardly bear more persuasive testimony. Surely a more united background for work than this seems to indicate is one of the first requirements for the advancement of any science. As a matter of fact the indispensable harmony among psychologists is supplied by the omnipresent peacemaker in two ways. In the first place, he tells us that after all there is no quarrel because the opposing parties merely study different aspects of the same actually existing data. The grain of truth in this contention is found precisely in the

[1] E. G. Boring, The Stimulus-Error, this JOURNAL, 1921, 32, 449-471.

fact that if our opposing psychologists are dealing with facts at all there is really only one set of such facts. But as in the case of most intercessors there is no profound examination of the difficulty involved between contending parties. Thus the problem is not considered (1) whether the psychological data have in fact the alleged aspects, or (2) whether they can be separated and separately studied. Now in order that anything in the field of science should be a datum it must be a unit object or occurrence, and so we assume that the mediators to whom we refer consider the field of psychology to involve different kinds of facts. Indeed it would be strange if such a large field as that of psychology did not contain many different types of facts even if all individual occurrences constituted the same form of datum. As a matter of fact, it is possible for different psychologists to choose, for their particular study, reactions which differ from each other as thinking and reflexes. But are such basic disagreements as we find in psychology today merely due to the fact that psychologists are dealing with one or the other type of responses? No, the quarrels concern the nature of the data themselves.

The mediators, in the second place, claim that the violent disagreements in psychology do not much matter since introspectionism and behaviorism, let us say, are different hypotheses concerning the data of the psychologist's domain, and it is not so very important, they say, that psychologists should agree upon their interpretations. Such disagreements (they further say) are found in physics, that most stable of sciences, in which light, for example, is sometimes interpreted in corpuscular and sometimes in radiational terms. Here again we must point out that the opposition would not long survive if the opponents were not inquiring into the nature of their data rather than offering an interpretation of them. Consequently an entirely different condition prevails in psychology from that in physics, in which science no question is entertained concerning the "data" of light, for example.

Now, as we have indicated, such marked differences of opinion as today exist in the domain of psychology are hardly conducive to anything but futile argument. So far indeed is such violent dissension from promoting any real comprehension of psychological facts that we ought not to take lightly the existence of such extreme views as those of the different varieties of introspectionism and behaviorism. Rather we should attempt to discover what in the facts of psychology themselves argues for agreement concerning the data, no matter how differently such data might be later interpreted.

II. Evidences of Possible Agreement in Psychology. Signs are many and important too that much general agreement

is possible and in fact gradually being arrived at with respect to the data and fundamental principles of psychology. Tremendously encouraging is the discovery that students of psychological phenomena, although starting from different theoretical presuppositions, when bent upon a serious investigation of actual facts, can come to much the same conclusions concerning the character of psychological data.

Now it so happens that the original psychological experiment, namely the study of the psychophysical response, is capable of affording us considerable insight into the nature of a psychological datum. The psychophysical experiment is, in the first place, a fairly simple form of psychological reaction which has been thoroughly and frequently discussed; consequently we should experience no difficulty in studying the crude facts with the hope of arriving at some fundamental psychological agreement. For we have full faith that if we can only get down to our crude data with a complete freedom from philosophic bias[2] an effective *rapprochement* between psychologists will not only be possible but to a certain extent inevitable.

The second reason for hoping to find a basis for the reconciliation of psychological opposites by a study of the psychophysical experiment lies in the fact that a genuine attempt to come to an agreement is already to be found in Boring's recent discussion of the psychophysical experiment to which we have already referred.

Not that Boring has set himself the problem of harmonizing his psychological point of view with that of others who hold different attitudes concerning the subject matter and method of psychology. On the contrary, the fact is that Boring's analysis of the psychophysical experiment is designed to substantiate the structuralist's position[3] as over against that of

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[2]Is this possible? We believe not. Consequently the question here really is whether some philosophic or cultural attitudes are more conducive to the obtainment of correct results in psychology than others. Our suggestion would be that we might very profitably give up all cultural backgrounds which commit us to the procedure of transforming the fact of pressing a telegraph key, when an agreed-upon colored paper is shown, into a mentalistic fact before we can describe it. From this standpoint we look upon the structuralist's reiteration of the distinction between "Kundgabe" and "Beschreibung" as a "defense mechanism." We discuss this problem in more detail later.

[3]Frankly we anticipate that the reader may say concerning the writer's present paper, "Well, here is another view to accept instead of our own." Our reply would be that we are not at this point interested in advocating some type of psychology rather than another, but rather we wish to raise the question whether psychology does not suffer from the fact that psychologists are committed to some preinterpretation or other of their facts before they are studied. Also, we wish to make the more positive suggestion that all interpretations should be derived from an investigation of non-prejudiced data.

the functional-mentalist, and the behaviorist who is presumed to be anti-mentalist. And so whatever symptoms of agreement we may find in Boring's discussion are not due to his desire to make peace with other partisans; yet overtures for agreement we do find in Boring's work nevertheless, and for the simple reason that he offers us an extremely valuable description of the psychophysical experiment.

In order then to test our hypothesis, that a critical study of the psychological experiment may lead to agreement between psychologists, the writer who stands not committed to either the introspective-structural or so-called orthodox behavioristic views,[4] and much less accepting any combination of the two, plans to make a comparison of the descriptions of a psychophysical experiment when made from the introspective-structural point of view and from what the writer assumes to be an objective point of view, based upon no other presupposition than that we are investigating some definitely observable fact. We shall therefore proceed to our comparative analysis of the psychophysical experiment.

III. The Analysis of the Psychophysical Experiment. Describing the two-point limen experiment, Boring says we have (1) stimuli in the form of compass-points at different separations. Next we have (2) perceptual patterns which pass through a series from a single sharp point to two separated points. These patterns Boring calls mental processes, or process material for the datum (structural) psychologist. Between stimulus and process there is "assumed" to be (3) an excitatory process (neural action). Finally there is (4) verbal report. This series of processes as a matter of detail Boring thinks it advisable to look upon as a dependent one, each phase a resultant of the preceding.

Reserving until later the question of the shapes of the mental processes we must next look into what appears to be a series of factors connected with the four above. In the visual schema which our author uses (p. 446) and which we hope the reader will consult, he places between stimulus and excitation the term 'stimulation,' saying: "stimulus gives rise to excitation by way of stimulation." We take this to mean merely that the stimulus or object gives rise to an excitation process by means of a stimulating act. Again, between the term excitation

[4]We sometimes wonder whether there exists such a person as an orthodox behaviorist (namely, one who thinks of the psychological datum as merely the action of muscles, nerves and glands) or whether he is a controversial artifact. If not, then the agreement of psychologists is more plausible then otherwise, and especially if it should turn out that there aren't any mentalists, that is, those who are committed to the view that the data of psychology are purely mental states.

and the term perceptual pattern, Boring puts the term 'attention,' and writes: "excitation culminates in perceptual pattern, under the selective action of attention." Now here Boring's meaning is not so clear; but we take him to mean that excitation is neural action or process and becomes transformed into or parallels a mental action or process. Finally, between the terms 'perceptual pattern' and 'report' Boring puts the term 'criterion,' with the following statement: "the perceptual pattern issues in a judgment in accordance with criteria of judgments that have been established." This statement we interpret to mean that the observer tactually perceives the stimulus and consequently is enabled to judge or say whether there was one or two points applied to the skin.

To us at least there appears to be a slight ambiguity in Boring's description, concerning which we might comment, since it is obviously essential that every item in our description must be carefully scrutinized. There are two points here. In the first place, Boring does not sharply distinguish what the experimenter does from what the observer does. The stimulation action is obviously that of the experimenter. To be attentive or inattentive is the action of the observer at the behest of the experimenter, as is also the work of observing the criterion. In the second place, there seems to be an attempt (at least verbally) to distinguish between two series of factors, one (stimulus-excitation-perceptual pattern-report) which does not stress action, while the other series (stimulation-attention-criterion) does. In the first series stimulus is certainly a thing and won't trouble us. Excitation is a condition and as an isolated process need not necessarily be called an action; report or judgment is clearly an action. But what of the perceptual pattern? Here we should like to have a more definite statement since our comparison of descriptions will hinge on this point. Criterion in the second series, although a noun in form, is, to judge from the context, without doubt meant to be an action.

Such then is the structural psychologist's analysis of the psychophysical experiment. Observe how an objectivist (let us assume that label for our present purpose) analyses the same situation. Our series of events runs as follows: we begin with (1) the stimulus; the next member of our series we call (2) the attention act. Then we have (3) a perceptual response followed by another act which we may call (4) an ideational, reflective, or judgment act. Then finally we shall have a final or consummatory act which we agree to call (5) verbal report.

Before attempting a comparison of our analysis with that of Boring we must point out that we divide our behavior situation into stimulus and response, the former consisting of the application of the compass-points to the cutaneous region in

question; in other words, the exertion of the pressure at a particular point. It happens to be true, of course, that in our experiment the stimulus phase of the situation involves the action of the experimenter, but this action is part of the stimulus. We may consider it as the setting of the stimulus, while the instrument-points we think of as the media of stimulation. According to the conditions of the psychophysical experiment as Boring describes them we must indicate that there is an additional or adjunct stimulus which consists of the instructions of the experimenter to be careful and to attend to the task at hand. Even without this addition it must be noted that the experiment would always involve two stimuli, one the pressure and the second the request to make a report. Were it not negligible for our present purposes we should have to include the warning to be careful as a second auxiliary or third stimulus. As it is we will try to keep the description as simple as possible. But we might add, however, that under certain conditions the first auxiliary stimulus we mentioned might be thought of as setting instead of stimulus.

The second phase of the act we call the response, and possibly in every case but a reflex action it consists of a pattern, namely the series (1) attention, (2) perception, (3) judgment and (4) verbal report. As to the interdependence of the members of the series we should agree that under definite experimental conditions and with a trained subject they may be practically dependent one upon the other, but it is certainly true that this is not always the case. For, as the experimental data all show, numerous errors in perception and judgment occur. That a wrong perceptual act (ordinarily called an illusion or a false judgment) should occur is entirely due we believe to the fact that what should be an organized progressive series of responses and stimuli do not operate in their expected order. It is more common of course in practice that the judgment should not follow its precurrent perceptual act than that the perceptual act should become dissociated from the stimulus. Under ordinary conditions also the verbal report might be expected to depend upon the judgment; but it might not, and certainly all kinds of slips can displace the connection of judgment and stimulus.

As to the nature of the response each member of the series is a definite adjustment of the person; in principle then all members are alike, being actions of the individual, but they differ in function and form. For us the verbal report is an act precisely like those called judgment, perception and attention; each consists of perhaps a single reaction system. We are especially eager to make this fact clear, for it is precisely in these details that the agreement or disagreement of psychologists

upon fundamental problems is possible. Our response, then, in the psychophysical experiment we may say consists of a series of reaction systems.

IV. Points of Agreement and Possible Agreement in the two Analyses. Unfortunately upon several points a complete harmony of interpretation between the two descriptions will not be found; but it is certainly true that upon essential points a fairly complete agreement exists. Let us proceed then to a consideration of the likenesses and differences of the two descriptions.

(1) A primary agreement and a most important one is that both undertake an analysis of the experiment. In both cases the reacting individual is studied in the sense that we may expect to be informed how he is stimulated, how he must conduct himself with respect to his attention to the problem in general, to his own action and to the stimulus. The importance of this point lies precisely in the fact that only upon such a basis can the student of psychology approach his data with an unbiased mind as to what these data are. We shall presently see that to look upon the reactions of the person as the data to be analyzed and otherwise investigated may be the means of avoiding unnecessary and harmful presuppositions.

(2) Another common result of the two analyses is the conclusion that the observer in the psychophysical experiment performs a series of definite acts or events, what we choose to call a pattern of response. The only question that may arise here is whether Boring would agree with us that the perceptual factor is an action of the person, in principle like the verbal report, but different from it as the perceptual act differs from an attention action. To this problem we plan to return presently.

(3) That the series of acts or events in a psychophysical experiment response can be divided into different functional parts is another very important point of similarity between the two descriptions. The parts are the end factor (vocal report) and the middle factors (attention, perception, judgment), called 'middle' because they lie between the report end-factor and the stimulus end-factor.[6] In the first place, such a distinction allows us to look upon the different members of the reaction series as merely functionally distinct, but not as different kinds of facts; and in the second place, it provides a basis for the individual study of the different factors.

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[6]Those who read this paper in conjunction with Boring's article (it was written with the assumption that it will be so read) will notice that I use terms which are not strictly mine or Boring's but rather derived from the two descriptions. I purpose, however, to guard carefully against ambiguities and especially against giving the impression that Boring accepts any interpretation of my own.

(4) We might turn now to a comparison of the specific members of the response series. To start with excitation. Here there is possibly a thoroughgoing disagreement between the two descriptions. For us excitation in the sense of neural action is not a separate factor but a constituent of every one of the four[6] reaction systems, to wit, attention, perception, judgment and report constituting the response. We do not conceive of any neural action alone preceding other kinds of action; but rather the neural action is a phase of the reaction system as a whole. If there is a deliberate disagreement between a structural view and an objective view here it is a very grave disagreement indeed, but we believe not inevitable. It is grave because, if the structuralist means to establish a permanent and actual difference between the neural fact and the mental fact (there is of course such a tradition), the structural position can never be reconciled with an objective point of view. That such a disagreement is not inevitable, however, we believe for several reasons. (1) There are psychologists who from an objectivist's standpoint are mentalists (Warren,[7] for example, who makes the two identical). (2) Boring himself must include a neural factor in several members of his series, attention and report for example. Whether he would care to admit this for perception we will discuss presently.

(5) Concerning the attention factor, this will not present us with any great difficulty, since attention being a fairly definite overt response the structural psychologist need not therefore differ so much from the objective psychologist as to what is involved in such action. We take Boring's inclusion of fatigue as a possible factor in the attention phase (p. 468) to support such a view. The same may be said when Boring undertakes to control attention by instructing the observer (p. 469). What else can "control by instruction" mean than that you ask the observer to do something of a somewhat different sort than to say "one" or to say "two?" Here we can certainly not avoid the fact that we are dealing with the mode of adaptation of our observer in the concrete psychophysical experiment. The writer is willing to make the mistake of assuming that no one will disagree that you can no more separate the mental from the rest of you when you obey the instructor's request to perform some particular act (speak) than when you are told to do something else(watch your action). In both cases you may attend to your actions besides performing them. Would it not be an unnecessary analytic sophistication to

[6]These types of reaction systems we should say, since the reaction systems themselves are of course abstracted from the actual event; there may be thousands of them actually operating.

[7]Human Psychology, 1919.

divide up the person into a mind or mental process on the one hand, and the interacting or paralleling bodily process on the other?

(6) Next we turn to the perceptual factor, which not only is the crucial point in our harmonizing enterprise, but is also cheerfully admitted by us to be a very difficult factor to handle. Both the structural and objective positions maintain or lose their value depending upon their ability to handle the problem of perception.

Because the perceptual response is fairly intangible, representing as it does in implicit form some previous overt response, and also because a perceptual reaction may represent a unique organization of vestiges of previously performed reactions, it takes on the character of being fairly uncontrollable and certainly difficult to observe either by the reacting person or someone else. Now it is in these ways that perceptual actions are different from the other more overt factors in the reaction pattern, and if the matter of observability is the criterion we may well agree that perception constitutes a relatively "central" fact. As we have already suggested possibly it is the presence in our psychological behavior equipment of such intangible reactions that lends such plausibility to a purely mentalistic conception. That is to say, we strongly feel that the idea that there exist in psychology different aspects of the same response to a stimulus, namely mental and behavioristic, is based upon the existence of the more refined perceptual and ideational response besides the grosser reactions, attention and so-called habit acts.

And now we may ask whether this admission of the difficulty in observing the perceptual factor because of its implicit character constitutes a basis for agreement of what is involved in the entire experiment. Or would a structuralist not agree in looking upon the perceptual factor as a complex reaction system, in principle precisely like the other factors? Our doubt here arises from Boring's statement that the perceptual pattern as he calls it has a shape or form (oval, elongated, double paddle, dumb-bell). While he calls this "process material," one gets the impression from reading his description that he does think in terms of material of some sort. The impression is deepened when our author writes that "excitation culminates in perceptual pattern." While we prefer (both in order to keep our minds open with respect to Boring's description as well as not to close the avenue to agreement) to hold that his statement means merely that the one fact is followed by another, the statement might be taken to imply that the perceptual factor is a kind of qualitative material or process transformed from neural material (current). At least the process that is involved

may be interpreted to be something other than the total action of the person, even if it be thought of as an action within him or within part of him.

If the structuralist can agree with the objective psychologist upon this proposition, namely that the perceptual factor is a reaction system, an adaptational act of the observer in the experiment, then the two points of view can be brought into substantial harmony. For our part, we cannot discover any insuperable obstacles to such an agreement, and in fact we believe that in the following considerations is contained convincing argument for such harmony.

In the first place, we are on common ground as long as the structuralists agree in rejecting the notion that a mental state or perceptual pattern is any kind of stuff or quality, in favor of the idea that they are dealing with processes or acts of the individual. Külpe calls this perceptual process sensitivity and sensible discrimination, implying that this element of psychological fact is an action of the person. When the perceptual factor is looked upon in this way the difference between the structuralist's view and that of the objective psychologist disappears. To us the entire perceptual problem seems clear when we are dealing with a complex action pattern which can be divided into separate attention, perceptual, final report and other reaction systems. But how about the case in which there is only a single reaction system? In that case attention and perception may be looked upon as integral phases of that reaction system. Perception is merely the cognitive or discriminative factor of a response system. From the standpoint of objective psychology the cognitive factor in a simple act, that is, one that involves a single reaction-system (reflex for example), is that peculiar coordination of response and stimulus which makes for a specific act. Cognition is the fact that different objects call out differential reactions in the organism.[8] In a complex act (many reaction systems) each one involves a cognitive factor of this sort, and besides one whole reaction system may be cognitive in its function.

In the second place, much encouragement for the possibility of reconciling the structuralist and objectivist do we find in the criticism made by the structural psychologist of the behaviorist and psychologist of capacity. In this criticism the structuralist implies that all he means to insist upon when he argues for a "central" factor in the psychophysical experiment is that he does not believe that in this experiment we merely undertake the physical measurement of bodily response as a function of the physical quantities of the stimulus. We may entirely agree with him that a bodily act (muscle and nerve

[8] Cf. a discussion of cognition in reflexes, this Journal, 33, 25f.

action) is not a psychological datum and that the experiment must involve other factors. But the question is whether the introspectivist agrees with us that this central factor, which is an essential one in the total situation, is a complex response of the actual person and not abstract qualities of mind or consciousness.

(7) Turning to the final item in the reaction pattern, namely the verbal report, we need only remark here that the agreement is complete. Without any doubt whatever, the experimenter can look upon the report as a reaction unit comprising all of the factors that can be specified as integral phases of such a system.

V. The Problem of the Stimulus-Error. And now we are prepared to examine briefly the problem of the stimulus-error and how that problem affects the general inquiry into the possible agreement between psychologists. Heartily in accord are we with Boring in his insistence that not the best scientific results can be obtained by studying merely the response and the verbal report, for there is more than these involved in the situation. Let us frankly assert, as we have already intimated, that the psychophysical experiment cannot be understood and much less controlled without taking into consideration all of the factors (reaction systems) involved. In so far then as the problem of the stimulus-error means that we must not overlook the total series of response factors we must recognize the problem and avoid the error.

Let the reader observe, however, that to agree to study the middle factors in a reaction pattern does not at all supply any new element or support for a structural position, since as a matter of fact even the psychologist of capacity (Cattell) never denied the existence of those middle terms. For Boring tells us that the psychologists of capacity call these middle factors, attention, perception and judgment, inscrutable "middle terms." What they do deny is that these factors can be controlled and measured.

Boring, in his study of the psychophysical experiment, makes two assumptions which he believes materially support a structuralistic (datum) psychology. In the first place, he assumes that if we recognize the stimulus error and avoid it we thereby shall prevent ourselves from committing the psychophysical experiment to equivocal results. The second assumption he makes is that since the control of the response factors or the middle terms of the response conditions the validity of the experiment the structural hypothesis is thereby established. Now for our part we may readily and cheerfully admit that we get different results from the experiment depending upon whether the observer is paying attention to his response or not, as Friedländer's experiments on weight-lifting appear definitely to

indicate; but does this imply anything at all concerning the mentalistic character of the middle terms? Moreover, suppose that it be established that in some cases when the observer pays attention to his reactions he can offer more consistent and more predictable results,[9] does this fact alone establish that the middle terms are mentalistic?

Furthermore, as we have already written in a previous part of the paper we believe most emphatically that there exists a distinct difference between the perceptual member of the middle factors and both the preceding attention factor and the final verbal report. This difference we consider to be in large part that the perceptual reaction system is much less overt and consequently more capable of direct observation by the acting person than by anyone else. Again, we may repeat that we are convinced that the perceptual factor of the middle terms is much less easy to control than are the other middle factors; but in neither of these two cases can we agree that any support is found for a mentalistic point of view. Nothing in these two facts lends credence to the interpretation of our data in such a way as to separate the structural psychologist from the behaviorist or from the capacity psychologist.

On the other hand, we do believe that any suggestions concerning the possibility of controlling the middle terms in the psychophysical experiment must imply that they are something other than mental stuff of any sort, in fact reaction systems. Much confirmation do we derive for this belief from the fact that Boring himself does not appear to insist upon our ability to control the perceptual factor but mainly the attention phase, and the attention factor is by far the least equivocal reaction system for him; that is to say, it is apparently psychologically least mental in his estimation. This last statement we make however in full view of the fact that Boring apparently groups all the middle factors as mental in contrast with the verbal report.

What kind of psychological principle do we commit ourselves to, we might ask ourselves, when we recognize the problem of the stimulus-error and attempt to avoid it? Why simply this, that we accept the scientific fact that psychologists deal with different kinds of data from those of physicists, for example. In brief, the psychologist is interested in psychological facts,

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[9]It is not at all scientifically established whether we get better results from attending to stimulus rather than to response in all cases. In our uncontrolled observation and especially in golf as well as other responses evidence seems rather conclusively to favor attention to stimulus.

to wit, the reactions called out by stimuli, that is the activities of the observer in the psychophysical experiment.[10]

At the risk of digressing from the main trend of our immediate exposition we must suggest that to consider the psychologist's data as responses to stimuli-objects, irrespective of how we regard the responses, is a gain toward a generally improved situation among psychologists and a step toward agreement. For we believe that the older mentalistic view that made the work of the psychologist consist of an analysis of the same objects from another angle from that which interested the physicist was certainly not correct. For example, Titchener believes that psychologists study the same objects that physicists study "with man left out;" but the former study those things "with man left in." The difference then between the two scientists it turns out amounts to this, in the view of the structural psychologist, that while the physicist studies the same objects as electromagnetic waves, vibratory motion or molecules, the psychologist studies them as looks and tones and feels, that is as sensations and feelings.[11] That the structuralist cannot be consistent in this attitude is plain when he adds to the sensations as psychological data feelings, thoughts, emotions, memories, imagination and volition,[12] for clearly those are the acts of persons. Moreover, the structuralist says that "the man left in" reduces to the nervous system, which makes his data closely connect up, if not identical, with reactions.

To the credit of the structuralist, it must of necessity be said that by making sensations and physical energy aspects of the same things he is attempting to avoid epistemological problems,

[10]We believe that at this juncture we get some light upon the argument of the anti-objectivists, to the effect that an objective psychologist has no right to assume any knowledge upon his part concerning his own responses. Because the objective psychologist assumes that he requires no idealistic epistemology in his work he is supposed to cut himself off from knowing anything about himself. The objectivist believes that when he observes his own reactions the knowing event is precisely as natural a phenomenon as when he responds to some other person's reaction. It so happens of course that when the psychologist experiments upon himself the responses that he observes are his own in two cases, to wit, (1) the reaction made when he offers the stimulus and (2) the reaction (his) to that stimulus; whereas when he experiments upon someone else he is interested only in the responses of the other person. The other person (observer) would, if the anti-objectivists were correct, be also cut off from knowing anything of his own behavior.

[11]Cf. Titchener, A Beginner's Psychology, 1915, 8ff.

[12]How the mentalist can hold such a view at all appears in our opinion to be accounted for by the fact that sensations are abstracted from the qualities of things and the other intellectual processes are made into derivations from sensations. Feelings of course remain outside this scheme and possibly this is the reason why psychologists have so strenuously attempted to make them into qualities or aspects of sensation.

but the difficulties he thereby creates are no less than those he overcomes. For he makes psychological data into peculiar abstractions as far as sensations are concerned, while the more complex reactions cannot be handled at all. And all this we believe can be rigorously avoided, as we have fully reiterated, by considering psychological data as responses to stimuli.

We come now to an extremely interesting phase of the stimulus-error discussion, namely the problem whether in fact the avoidance of the error means any slighter attention given to the stimulus even when one greatly increases his control over his own reactions to the stimuli in question. Interest attaches to this point because we insist that the control of the response involves at the same time a greater control over the stimulus. How can it be otherwise? For we believe most firmly that the psychological fact is the interaction of a response with a stimulus.

That the control of the response in the psychophysical experiment is at the same time a control of the stimulus, or at least involves prominently the stimulus, is evident when we consider that the errors in the experiment are not so much due to the neglect of the reaction as they are to the fact that the observer shifts his attention from his action to the stimulus and back again to his action. The only question here is whether a better control over the situation could be obtained by paying attention exclusively to the reaction, after paying attention to and perceiving the stimulus of course. In answer to this question we are not ready to say Yes, but we are thoroughly convinced that in any case what we are dealing with in psychology is a reaction to a specific stimulus and possibly we must conclude that it is always essential for both stimulus and response to be synchronously controlled.

Assuredly we find various evidences for the impossibility of minimizing the importance of the stimulus in the psychophysical experiment, and one of those is the fact that because the perceptual factor cannot be readily controlled, if at all, we must, when the problem of controlling responses is in question, place our emphasis upon the attention factor and the verbal report. Very significant is this fact since both of these factors are very definitely related to the stimuli factors of the experiment. Furthermore, unless we keep the stimulus under control as well as the reaction factors we cannot achieve any capacity to predict what responses will be made to stimuli. That this prediction must always continue to be very imperfect is due no doubt to an inability to control the perceptual process.

With the structuralist we believe we share this idea of the importance of the stimuli factors in psychophysical experiments, for Boring's insistence upon the control of the attention

factor may be definitely interpreted to stress this point. Also we find Boring's attitude favorable to our view in his belief that the control of the psychophysical experiment can be achieved by means of a criterion. For a criterion cannot be thought of as operating exclusively with a perceptual pattern but only in conjunction with the stimulus.

Let us notice that when we plan to control our responses to stimuli what we can do is to compare our own report, based upon our present judgment concerning what we have perceived with respect to the stimulus, with what we have judged a minute ago. But certainly it seems clear that we cannot control the kind of perceptual response we should make to any particular stimulus. Our control is bound to connect itself with more overt factors in the behavior segment than the perceptual reaction. Let us repeat, then, that what we do while paying attention to the response rather than to the stimulus consists of more carefully controlling our report with respect to the stimulus. Notice that we do not in any sense give up the stimulus, nor attach ourselves exclusively to the response, for the response doesn't mean anything without the stimulus. The condition might be entirely different, however, if we could assume that there is in us some quality which can be seen or otherwise known; in such a case we might consider that we could study it. But this assumption in our opinion could only rest upon an interpretation added to our observation in order to support a structuralistic position.

And now we hasten to add that nothing that we have written implies our rejection of the hypothesis that a verbal report follows a judgment response and that the judgment is based upon a perceptual response. What we do insist upon is that each factor constitutes a reaction system, a positive response of the person, and that all the response factors together make up a single reaction pattern to the specific stimulus in question with its experimental setting. To attempt to control the psychophysical experiment by attending to both stimulus and response results, in our opinion, in a closer connection between the perceptual factor and the stimulus.

VI. *The Data of Psychology.* We believe that the comparative analysis that we have made of the descriptions of the psychophysical experiment forces us to recognize that the data of psychology cannot be thought of as being anything else but actual responses of a person to specific stimuli. Any other way of describing the situation cannot be said to be an analysis of the facts in the case. Surely the employment of such terms as mental, physical or bodily, when intended as descriptive names, constitutes interpretations based upon some kind of assumption or set of assumptions. These assumptions we may

further say are not based upon observable facts whether found in this experimental situation or elsewhere. Rather, they are derived from some prepsychological or perhaps even prescientific domain of human thought.

Our analysis of the psychophysical experiment, which be it remembered we take to be in principle typical of psychological phenomena, has yielded, in our opinion, other results which locate the actual facts in the case and provide a basis for the agreement of psychologists. One of these results is that we may consider the psychological reaction to be made up of comparatively simpler unit reactions or that a large behavior or response pattern can be analyzed into smaller unit reactions.

Another fact concerning the data of psychology which we may accept as an actual product of our analysis is that some of the unit reactions in the psychophysical experiment are primarily overt responses while some are mainly implicit. In the latter instance the facts are in part not entirely observable and consequently involve some assumption. But here again the interpretation is we believe absolutely derived from the observed data and is essential, at the same time that it is supplementary.

In support of our contention that the data of psychology are in fact complex responses of organisms to things and not mental states or aspects of things, we may adduce the fact that in spite of psychologists' contrary theories their practice has always been based upon a reaction hypothesis. Take any book written by a professed structuralist and what do we find? There is not a single psychological activity which is described other than as a response to a stimulating condition. This is certainly true, is it not, of the simpler facts treated under the heading of reaction time? And when we come to sensations are not these always presumed to be abstractions from the adjustments of organisms, abstractions consisting of the differential factors removed from the rest of the adjustment action? This we assume is what is meant in part by psychologists who consider sensations as merely abstractions from perceptual acts, or when they consider sensations as correlates of receptor and neural functions. Moreover, unless the structuralist considers sensations to be phases of reactions would he not be inevitably and hopelessly committed to stimuli-errors? We come, then, to the conclusion that the interpretation of psychological data as anything other than responses to stimuli is the result of an influence extraneous to psychological analyses and in fact is the outcome of peculiar philosophic preconceptions. It is hardly too much to say that the entire psychophysical problem with the bitter battle it entailed concerning the quantification of mind was due to a preinterpretation of the

facts prior to a fair study of them. What makes the situation more difficult than it otherwise would be is that philosophic preconceptions are not knowingly accepted but merely taken over with the rest of the cultural traditions of the time. This condition prevails to a certain extent today and was the dominant circumstance in the earlier days of scientific psychology.[13]

Does not the very term psychophysics stand as a symbol that the work and method of scientific psychology was prejudiced at the outset? And was it not this prejudice that was responsible for the unending strife concerning the stimulus-error and whether what was assumed to be the psychological datum could be measured?

From the standpoint of the reaction hypothesis it is almost tragic how the various students of the psychophysical experiment have misinterpreted the more implicit elements of the reaction system of the psychophysical reaction and thus precipitated the controversies as to what was measured or whether what they wanted to measure could be measured. And so as to the identity of the datum; it was either made into something that manifested itself in action (verbal report, Fechner) or it was presumed to be a parallel or manifestation of nervous action (G. E. Muller), or again the datum was transformed into a judgment (is this not true of most psychophysicists?). In every case we believe the actual thing studied, namely, some part or the whole of the reaction to a stimulus, was interpreted to be something other than it actually is instead of being described as an existing fact. And most unfortunate it is that scientific or experimental psychology still carries with it the evil effects of not having been begun as a critical investigation of concrete facts.

VII. How Metaphysics Influenced Psychological Interpretations. Most clearly can we see the modus operandi of the philosophical background in experimental psychology when we look back upon the early development of the psychophysical experiment and scientific psychology in general. Now what do we find? At the beginning of the development of scientific psychology, when "psychophysics" as a technical scientific term was developed, the primary philosophical interests of Europe were still concerned very definitely with the epistemological problem how the process of knowing can mediate

[13]We wish to eschew absolutely the implication that we believe that the mistakes in early psychological work, if such we admit, could have been prevented, or that some individual or group of individuals is responsible for whatever difficulties one admits to be present in the early experimental work. On the contrary, we believe that the conditions attending Fechner's work made that work historically inevitable. But our whole point is, must we forever burden psychological science with errors that crept into it in its infancy?

between a known material object and its psychic correlate, and the question how the knowing process can reproduce in consciousness what is apparently a totally different form of thing. This interest, of course, arises from the theologico-metaphysical background which implies a diremption between spirit and matter, mind and body, and the mental and the physical. It was in the interest of this metaphysical problem and not in order to investigate a definite psychological problem of response or adaptation to a stimulus that Fechner undertook his work on the psychophysical problem.

In point of fact, Fechner entered upon his activities with regard to the psychophysical problem in an endeavor to establish definitely the identity of mind and matter (by the employment of the Herbartian suggestions of mathematical expression and his threshold mechanics) and not to investigate any psychological problem.[14] As a result his work was the target for a host of criticisms which could only be effective because Fechner sought to establish his religious or metaphysical theory by the utilization of physiological data. In other words, we might say that Fechner through his metaphysical interest prevented a more definite and more scientific interpretation of the sensory psychological data available and a more substantial research for more data.

But if the scientists who came after Fechner were more interested in the psychological facts than in the establishment of a metaphysical theory, their work still suffered from much the same philosophical bias. Though psychologists undertook to study assiduously the facts of association, memory and perception they did so with the assumption that they were bringing together the psychical and the physical. Experimental psychology, especially, consisted of bringing together physiological data with facts of knowing, feeling and willing which are in some sense different from the former. What was the result? In the case of Wundt's psychology (to mention one example which has had far-reaching effect upon current psychological thinking) the metaphysical influence was responsible for the misguiding of a voluntaristic point of view, which originally promised to study psychological phenomena as derived from a biological matrix, that is, from observable facts, into a hopeless intellectualistic abstractionism which Wundt himself deplored in Herbart and his predecessors.[15]

To our way of thinking the whole gamut of evil results brought on by the metaphysical influence upon psychology

[14] Says Wundt, in his address on Fechner (Gustav Theodor Fechner, 1901, 84): "Das ganze Interesse Fechners gehört eben nicht der Psychologie als solcher an, sondern diese ist für ihn nur ein Bestandtheil der Natur- und Religionsphilosophie," etc.

[15] Cf. Wundt-Titchener, Principles of Physiological Psychology, 25ff.

is that psychologists can still think of themselves as dealing with epiphenomena.[16] And just here can we find one of the most serious evils of attempting to interpret psychological facts in metaphysical terms. Since there can be no possible relationship between the observed facts and the interpretations, the facts are not interpreted at all and thus are blindly handled or else they are misinterpreted and in consequence are far removed from any progressive scientific development.

Is it too much to believe that if psychologists had started out from the beginning of their science with the study of the psychophysical experiment or the reaction time experiment as an existing fact they never would have made the division between mentalism and behaviorism or between mind and body? In our opinion this particular division is a cultural heritage from a definitely pre-scientific period of intellectual development; possibly it is merely a theological conception. The writer would suggest that in the present stage of scientific attainment we need have no such dualistic or monistic conceptions as the mental-physical dichotomy seems to indicate. Emphatically we may suggest again that to reject all philosophical preconceptions means that not only shall we not lose thereby the distinction between psychological and non-psychological facts but on the contrary we shall achieve a much more sensible form of distinction.

The untoward influence of metaphysics upon psychology has really been twofold, the first of which, to wit that the datum of psychology comes to be considered in theory at least not as a reaction to a stimulus, we have already studied. Now the second deplorable effect of spiritualistic philosophy upon psychology has been such that, even where the psychological fact is either in theory or practice considered as a reaction to a stimulus, the reaction is not looked upon as a concrete reaction or adaptation of the natural organism, but it must be interpreted as a mental or spiritual thing or process (epiphenomenon). We may turn now to an examination of the second of these misinterpretations of psychological data, which compels us to interpret them as anything but complex organismic responses to stimuli.

And first we might consider the peculiarly intimate character of human psychological phenomena. Every psychological reaction is in a definite sense a very particular kind of event; it is the action of a human individual, a person with a unique value of his own. This fact is recognized in one of the cardinal attitudes in psychology, namely, that the fact of individual differences in the cornerstone of the science of psychology.

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[16] Cf. Washburn, Introspection as an Objective Method, *Psych. Review*, 29, 1922, 89ff.

Moreover, we may well grant that much as we may know concerning the response of another person to a stimulus, that action is in a unique sense bound up with that person and his past experience. But yet this fact does not in any sense support a mentalistic interpretation; in fact we believe it even argues for an objectivistic interpretation.

Does the intimacy of an act make it anything other than a natural phenomenon? To answer this question we must first ask what we mean by intimacy. Do we imply anything more than that in a given time and place some specified event has happened to some particular individual? Now unless we especially desire to give our data a particular interpretative coloring we cannot overlook the fact which the events themselves present us with, namely, that if the event happened to a stone or any other physical object the event would be no less a unique occurrence of a natural fact. As a detail of scientific methodology the more accurately one can determine the spatial or temporal setting of an event the more elements one has to add to an objective study of the phenomenon no matter what particular sort it may happen to be.

A second characteristic of psychological facts which probably lends plausibility to the mentalistic interpretation of psychological data is their fleeting character. Because psychological facts are intermittent processes of brief duration and not subject to reappearance they are considered as different from other natural events; they are considered as psychic and not physical. That psychological facts are fleeting actions, that is again to say unique events, cannot be denied, but that this fact involves any sort of different situation than any other natural fact is preposterous to believe. Is it possible to revive any event, let us say the particular chemical reaction which I have just performed? It is true, of course, that some kinds of facts are so comparatively simple that we may, for practical purposes, say the securing of measurements, consider them as the same, although they are different events. But is this circumstance any different in the field of psychology from what it is in that of the other sciences? That even the simple physical facts are never the same and cannot be exactly repeated we learn from the pervasive and necessitous employment of the statistical method in the physical sciences.

Again the striking fact that some of the data of the physical sciences involve reversible processes may have strengthened the philosophical influence upon psychology. Some chemical reactions for instance are almost reversible, that is to say, such similarity is found in the things transformed or retransformed that the same event may be said to occur. An examination of this case impresses us with the fact that even if the measurements made give us the same results without averaging, which

of course is hardly the case, we could not consider these as repetitions of the same event without abstracting from the temporal variable. Furthermore, is there so much difference after all between the reversible chemical action and some forms of psychological phenomena? Certainly any language or learning situation shows a reversible reaction and a repetitive one.

Possibly by far the most potent source of mentalistic interpretation can be traced to the facts of self-observation or introspection. By this we mean that in human reactions there are certain ones which cannot be observed by anyone but the acting individual. Let us take a concrete case. From my window I see some person come out of the next building, walk about a hundred paces to the right, then turn around and walk back into the building. Soon the individual reappears with several volumes under his arm. We may be fairly certain that what we have here is a memory reaction. Apparently the person forgot to take the books he planned to take when he first left the building. Of course it is barely possible that this is not a memory act, since it might be true that the person did not go back for the books but for some other purpose. Seeing them, however, he decided to take them home. But at any rate, and this is the important point, the fact that this action may from the standpoint of the observer fall under two different classes, and for the reason that the observer cannot tell what the stimulus is, indicates the dependence of the description upon the reacting person.

Just what is the adequate stimulus in many cases the onlooker cannot know, even when he is sure what sort of reaction situation is being worked out. Suppose we put our illustrative individual under controlled conditions so that we may know he is performing a memorial reaction. Now in the case we have chosen, what is the stimulus that evokes the memorial reaction? It is possible that the person is performing the memorial response because he was stimulated by some books carried by another person. These books may serve as a substitute stimulus for some books he wishes to carry home, which books constitute the adjustment stimulus. On the other hand, it may be that this individual was stimulated by looking at a tree. The tree may operate as a substitute stimulus because one of the forgotten books contains some information on trees which the individual is interested in. Thoroughly to understand the behavior situation it seems necessary to know what the stimulus is in this case, and yet we cannot normally have that information. Again, in some cases the stimulus cannot be known to any one but the reacting person. This necessity of knowing what is the stimulus, let it be observed, exists only for us who wish to describe and understand the reaction and not in order that it be possible for the reaction to occur. It is

of course probable that while the memory reaction cannot occur without the substitute stimulus, the reacting person himself may not know what it is; it merely occurs to him, so far as he can tell, that he must return for the books.

What is true of the onlooker's knowledge of the stimulus phase of our illustrative behavior is always true perhaps concerning some phase of the reaction. But we question whether it is anything but a metaphysical presupposition which on this account takes the memorial act out of the domain of natural phenomena. For how many factors in the events which exclusively involve purely chemical and physical behavior are we always ignorant of?

And is not precisely this the function of the scientist, namely, to discover as many of these conditioning factors as he can in any kind of event, in order to give definiteness and authenticity to his observations and subsequent interpretations? The mere fact that we do not know what are all the conditions in this person's action certainly doesn't transform it into anything other than an objective event. This fact is obvious if we consider that the same situation prevails with respect to man's digestive conditions. Is it any less a purely objective fact that man digests food and a particular kind of food, if only the digesting person knows how and when this particular event is happening?[17]

But we have yet to face the problem of the cognitional uniqueness of human reactions. That is to say, a very distinct difference may be observed between what we call physical or natural events and what we term psychological facts in that the latter involve a knowing element. Not only does the person react to a stimulus, but he may know he is doing so.

In the first place, the question arises whether the fact of knowing is a mark of difference between psychology and other kinds of facts, because, as we have just indicated, many features of the individual's response may be unknown to himself, and this refers not merely to the stimulus side of the factors but to the response side also. Here we need only refer to the occurrence of reflexes and a host of responses popularly called subconscious and unconscious. As a matter of fact, then, the knowing of the action by the person who performs it is not an essential factor in the event; and therefore a psychological

[17] Although Washburn (*loc. cit.*) assumes that her discussion of introspection supports an epiphenomenalism, the objectivist cannot be aught but encouraged by it, since even a professed structuralist means nothing more by introspection than objective description. Washburn's epiphenomenalism turns out to be, then, a matter of arbitrary preference, based perhaps as she suggests upon early training. We must, however, look upon her paper as a step toward the meeting of extremes.

event from this standpoint cannot be interpreted as a mental thing different from what is called a physical thing when that term is synonomous with a natural event.

But then we do have, of course, responses in which knowing is an essential factor, and our inquiry may be directed toward the question whether knowing itself as a fact is mentalistic in the sense of a non-naturalistic phenomenon. Before asserting our disbelief in this proposition let us ask whether we can get some light on this problem by inquiring why knowing is presumed to be mentalistic. This inquiry is stimulated by the fact that there appears an obvious difference between the act of picking up an object, say, and the act of intending or desiring to pick it up. Here again we must ask why the apparently "internal" implicit act is different in principle from any other kind of action. We are firmly convinced that the knowing or internal act is distinctly a response to a stimulus just as the picking up response, and moreover, a response to the same stimulus. Now the difference between them is that one we look upon as precurrent while we consider the other to be a final response.

The hiddenness of an act should be no inducement to think of it as a mental thing because certainly nothing could be more hidden from observation than the act of digestion. Obviously, we can know both events, at least in some cases, by particular means of experimentation. There yet remains the fact that the knowing is the action of the same individual who performs the final act. This point does not add anything more novel to the situation than the points we have already discussed. Our point is that all of the acts of the person, the knowing and the final overt (motor) responses, are purely definite natural events that happen to that particular individual.

VIII. How the Data of Psychology compare with the Data of the Physical Sciences. Remains yet the question, if the data of psychology are considered to be natural facts in principle like the data of the other sciences, wherein then lies their specific differentiation from the latter? So far as scientific investigations are concerned we may say that all scientists are interested in particular kinds of natural objects or things and the way they behave. For our present purposes[18] we might classify scientific facts into three general types, namely, physi-

[18]Our present purposes are such that we neglect the fundamental principle that there are as many kinds of scientific facts as there are scientific interests and information.

cal,[19] biological and psychological. The physicist studies actions of physical things (leaves, stones, dynamos, light, etc.) and the actions of these things he finds he can sum up under various laws, the Newtonian laws in mechanics for example.

The biologist studies actions of things (organisms) which have different qualities from the objects which occupy the attention of the physicist. Biological things behave on the basis of stored up energy (action), facts which may be symbolized by the terms metabolism, irritability, etc.

Psychologists likewise study a type of natural thing, namely some person or animal.[20] What is of interest is that the psychologist studies behavior which differs in various definite ways from the behavior classed under the two preceding types.

For practical purposes we may attempt to sum up the essential psychological character of behavior under the six following headings, each involving a different relation or interaction of a stimulus and response: (1) variability of reaction, (2) differentiation of reaction, (3) modifiability of reaction, (4) integration of responses, (5) delay of reaction, (6) inhibition of reaction.

(1) *Variability of Reaction.* When a psychological organism comes into the range of stimulation by an object, the relation or interaction between the two continues to a definite conclusion, frequently with a great variety of movements upon the part of the organism until one of several things happens. That is to say, until the organism (*a*) changes its relation with respect to the stimulus; (*b*) produces some effect upon it; or (*c*) the stimulus disappears or becomes invisible; or (*d*) the animal is fatigued or exhausted. The number and intenisty of these variations are determined of course by the specific details of the situation. (2) *Differentiation of reactions.* At the basis of the variabilty of behavior as well as of some other marked action characteristics is the process of differential reaction. In a general sense, we may say that different specific objects and persons or qualities of them produce different effects upon the organism, which in turn exhibits a differentiative sensitivity toward those objects and qualities. (3) *Modifiability of reactions.* In general the characteristic of modifiability of

[19]Here we must make sure that our language does not betray us into a nest of difficulties. The term 'physical' is here used in a technical sense as the domain of the physicist. When the term 'physical' is taken to mean natural as over against the metaphysical world of epiphenomena psychology, of course, constitutes a branch of the physical sciences. If the writer may offer his personal opinion the world of epiphenomena is nothing but a series of verbal constructions.

[20]From a psychological standpoint the thing studied may be considered as primarily a series of reaction systems to specific stimuli.

response permits the organism to profit by its previous contacts with those objects and in consequence allows the individual to develop a growing power over his surroundings. Thus we might say that the modifiability of behavior is a genuinely acquisitive process; the child, for example, who modifies his original hand-extension reaction to the candle flame acquires a withdrawal response or in other words learns to withdraw or not to extend his hand into the flame. We may think then of the characteristic of modifiability as a discrimination of stimuli, discrimination conditioned by previous reactions to those stimuli and also by present variation in the environmental settings of those stimuli objects. (4) *Integration of Responses.* Not only does an organism perform differential reactions to objects and modify those reactions but it also summates these responses. The psychological organism develops complex response systems which can be definitely observed to be combinations of simple reactions. As an example of this integration we might consider the development of speech. At first the infant learns to make single verbal responses to objects. Later the single word responses become integrated into sentences or comprehensive language reactions. (5) *Delay of Reactions.* This is brought about in two ways: first, the stimulus excites a reaction system or series of reaction systems which function incipiently until such a time as an overt response can be performed. Let us observe at once that the incipient acts may be overt responses in the sense that some direct action is performed upon the stimulus-object, but the final overt act is the significant one. Thus, in the case of a delayed reaction of an elementary sort, some overt act in the form of a posture, or bodily set or orientation, will serve to fill in the gap between the stimulation and the final response. Or secondly, some object other than the original stimulus but which substitutes for it can at some later time excite the person to respond to the original object. In the first case, the various positive attitudes of the organism may serve as preliminary reactions to the final act; they are then called precurrent responses. In the second case, there is an actual discontinuity in the behavior of the organism. Both of these forms of delayed reactions constitute phases of memory behavior, the latter of which is of course the more complex and the more serviceable to the organism which is able to react in that way. (6) *Inhibition of Reaction.* Another characteristic of psychological action is the inhibitory process. Let it be carefully noted here that by inhibition we do not mean non-action but merely preferential reaction. That is, there is a differential and preferential operation of the various reaction systems which the organism has acquired. For various reasons some stimuli may become prepotent over others, as in the case

of a more favorable setting which they may have, or because they have just operated upon the organism which is now especially sensitive to them.

So far as human psychology is concerned we may say without hesitation that the datum is a response of a person. Now if we agree to dispense with any kind of metaphysical implication we mean by person nothing other than what is implied in our everyday speech. We do not go behind the person to a metaphysical substance called the soul or mind, nor do we reduce him to the abstraction of a body. Rather we consider him as a complete psychological machine operating in contact with stimuli exactly in the same way as the physicist considers a dynamo.

Recent investigations made with human individuals both normal and abnormal have resulted in a conception of personality which shows fair promise of developing into a point of view capable of doing justice to all of the complex and intricate facts of human adjustment. The analysis of this individual in action yields us much information concerning both the simpler and more complex unit reactions, which under the name of reaction systems are abstracted from the actual behavior of the person in his surroundings. In our estimation the psychological conception of personality refers to the particular group and sum of reaction systems that can be described as adaptable or maintenance reactions in their simpler forms and as economic, aesthetic, literary, cultural, intellectual, moral, etc., when the reactions are very complex.

The merit of these abstractions lies merely in the fact that they are directly derived from the observable behavior of the person and they can be further reduced for more minute investigations to the simpler muscular, neural, discrimination and other functions of which they may be said to be composed.

IX. How can Psychology be Emancipated from Metaphysics? That psychology needs to be relieved from its entangling alliance with spiritualistic philosophy is manifest from the unfortunate results which accrue from such a connection. As we have been attempting to point out, psychology's contact with metaphysics may be summed up in the proposition that the data of the science have been taken out of their actual concrete setting and transformed into various sorts of abstractions. This amounts to nothing less in practice than that the data of psychology are prejudiced before they are studied. The method of doing this has been as follows. Starting with the assumption that our stimuli consist of radiation of various sorts, psychologists have had to suppos[illegible] [illegible]olors, tones and other qualities of natural things co[illegible] [illegible]r transformed in the mind, from the light ra[illegible] [illegible]s of a

nervous system. Consequently the psychological reaction had to be reduced to the process of "being qualities" (sensations). This attitude was formulated in the statement that the "mind" consists of these processes (sensations), etc., as over against the body and the stimuli. That the nervous system could be spared from reduction to radiations and mental qualities was only made possible by the psychologist becoming contemptuous at this point of the self-same metaphysics which brought on the problem. In the meantime, of course, psychology is not occupied with its own essential problem, namely the adaptations of persons to things.

And yet what appears to be the psychologists's deplorable occupation with the abstractions which we have been discussing is not without the encouraging feature that the whole procedure represents an attempt to place psychology upon a critical scientific foundation. In fact, this whole tradition of abstract psychology has been developed especially by Wundt and his followers in an admirable scientific spirit. And so psychologists seized upon what at the time of experimental psychology's birth were definitely established scientific facts, namely light rays, etc. The unfortunate feature of the situation, however, was that the metaphysical influence urged psychologists to correlate the abstractions of physics, taken to be the material world, with comparable mental abstractions, namely sensations, which were assumed to be the mental correlates. Because this was all done in order to make psychology into a science, however, the encouraging fact remained, namely, that the way was never closed to a modification of point of view which we believe is now making rapid headway.

Once more let us emphasize this point, that the abstractions we have mentioned, although necessary and valid for the physicist, cut the psychologist off from the study of the concrete reactions of human and infrahuman organisms to air, water, food, mates, art, science, etc. Are not light rays, air waves etc., in general very remote from actual psychological facts? For we assume that it is light and not light rays that have to do with stimulation, and furthermore light itself does not ordinarily constitute stimuli but is really the medium of contact or stimulation. Of a surety light and light rays may be stimuli-objects for the physicist when he studies them; but this is a special sort of psychological fact. Most certain it is that no science can go far without abstractions, without cutting its data up into bits which may little resemble the original wholes, but our entire insistence is that so far as psychology goes the gross

data should be the facts of the psychological domain itself.[21] For otherwise we may borrow abstractions in quantities from physics, from chemistry, and from other sciences, but they will serve not one whit to illumine our problems or to aid in solving them; since the facts of psychology, as we have seen, are clearly distinct from those of physics and the other sciences.[22]

Let the reader be warned that we do not base our argument at any point upon the necessity for achieving practical results in psychology. We are not interested for the moment in applying our scientific principles, although it is undoubtedly true that the sounder our principles are the better results can be achieved when we do apply them. Every psychologist must be struck with the success of certain features of the Freudian psychopathologists, those features we mean which have undoubtedly thrown much light upon psychological phenomena. No, our suggestion is made entirely for purposes of understanding psychological facts; for purposes of analyzing our facts we ought to approach nearer the actual occurrences. Let it not be said that the technical psychologist understands much less about human beings and their behavior than novelists or students of hysteria. In this connection we might suggest how much better off psychologists would be if they paid more attention to the concrete conditions of memory and thinking (as some psychologists are beginning to do) than to anything that has to do with the mental and the physical. For we may plainly say that there is nothing in nature which is mental or which is physical in any other sense that that natural phenomena can be divided into psychological and physical phenomena. Scientific psychology, in common with all the other sciences, selects its facts from the common source of natural phenomena and these facts comprise physical reactions, biological and geological facts, psychological responses, etc. The only use we can make of metaphysical abstractions is that by their employment we may dissolve facts into metaphysical concepts which we can in turn use to bolster up the metaphysical background of our science. Should such a background be tolerated?

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[21] This point refers to all abstractions: we are no better off when we take our abstractions from the biological domain than from that of physics. The so-called orthodox behaviorist takes his abstractions from the biological world and for this reason appears to keep closer to actual facts than the introspectionist who denies that sensations (color and other qualities) are abstracted from objects. For muscle action, nerve action, etc., are clearly abstracted from observable data. But even so the behavioristic abstractions are not of great value in psychological investigations.

[22] It is indeed gratifying to read in the newer textbooks overt statements with respect to the distinctness of the scientific field of psychology. Cf. Woodworth's treatment of color reactions in his *Psychology*, 1921, 210 ff.

Most firmly do we believe that all scientists theoretically appreciate the disservice brought to science by metaphysical presuppositions, and also that it is common knowledge that no scientific enterprise can be carried on without some fundamental assumption. We are in duty bound, then, to draw up a closer bill of particulars in our protest against the metaphysical wrongs committed in psychology. Let us specify, therefore, that we mean to rid psychology of metaphysical or transcendent philosophies and complete the emancipation of our science by retaining only or developing a logic or methodology of science. What would be the difference then between these two? Nothing less than this, that instead of entertaining presuppositions concerning the nature or identity of things before anything is studied, the psychologists will develop assumptions concerning the operation and meaning of psychological facts actually observed. Only from such assumptions and hypotheses may we hope to develop methods of discovering new data and newer methods of studying them. Incalculable would have been and could still be the application of such methodological principles in the study of instincts, emotions and intelligence, as the history of these problems amply testifies.

But here again we are entirely willing to agree that our suggestion contains nothing new. In fact Wundt makes a great deal of metaphysical presuppositions and methodological processes, but the question is whether his suggestion is in fact acted upon. The criterion lies here. When I perform a psychological act is my action divisible into a physiological part and a psychological part? We believe not, when we approach the fact without prejudice. That psychologists can agree upon this is eloquently testified to by the following quotation from a writer whose psychological position stands at the opposite pole from that of the present writer. Says Wundt: "the life of an organism is really one; complex it is true, but still unitary."[23] In spite of such a statement the metaphysical influence is responsible for the division of an action into mental and physical parts. We have then the result that while Wundt looks upon the mental processes (mind) and the physical processes (neural action, etc.) as conceptions, that is scientific interpretations of the reaction, he soon makes psychology into the science of mental states which are clearly different sorts of facts from the neural, although he still insists they are uniformly "connected" with those neural facts. Wundt's scientific path is here made smoother by the fact that according to his philosophical position both the neural and the psychic facts are ultimately reducible to homogeneous spiritistic stuff. Furthermore, now that the reaction is divided into physical and mental the same division

[23]Physiological Psychology, 1.

is made to apply to stimuli objects, and a (perceived) tree, for example, is made to consist of a natural science part and conscious content part. The total results of this situation are in our opinion inimical to the interests of scientific psychology.

X. *Conclusion*. In concluding our paper we might briefly summarize some of the salient points that we have attempted to investigate.

In general, we might conclude that it is most essential for the purpose of psychology that the various workers should agree upon fundamental propositions. For in psychology, as is not true in other sciences, the disagreements go back to the primary data of the science, the things observed, and do not involve merely interpretations of things studied. As a consequence, psychologists not agreeing upon their fundamental data are much occupied with preinterpretations which for the most part unwittingly play havoc with both the facts and the science.

That psychologists might agree upon their data and primary methods we have concluded from a comparative study of a subjective and objective analysis of the psychophysical experiment. In each case the facts studied are first approached as definite responses to carefully controlled stimuli. Possibly in any careful study much the same agreement could be accomplished with respect to interpretations, but this condition, while very desirable in the interest of the science as a whole, is not of equal moment with the necessity of agreement with respect to fundamental data.

On inquiring as to the conditions which make possible such violent disagreements as we find in the domain of psychology, we may trace the difficulty back to a traditional acceptance of a faulty philosophical background. To divide an organism's action into mental and physical and to make the data of psychology into anything but responses to stimuli is not a scientific enterprise but a metaphysical one. The way to clear psychology of such metaphysical obstacles is to eliminate all transformative presuppositions and to make the hypotheses and assumptions refer to methods of investigation and interpretation rather than to variant existences.

MOVEMENTS OF PURSUIT AND AVOIDANCE AS EXPRESSIONS OF SIMPLE FEELING

By Paul Thomas Young, University of Illinois

In a previous experimental study[1] (conducted at the University of Minnesota) it was concluded that unpleasantness is associated with a widespread bodily response while pleasantness is lacking, for the most part, in organic and kinaesthetic processes. "With U there are withdrawing movements, frowning, straining, reflexes of expulsion, etc., while P is characterized by mere acceptance of the situation and the passive yielding to it......P is felt when one relaxes, or simply 'does nothing.'" The data furnished abundant evidence for the correlation between U and movements of avoidance; but they cast a shadow of doubt upon the generally accepted correlation between P and movements of pursuit.

In a recent study[2] Corwin has advanced the criticism that our failure to find seeking movements as involuntary expressions of pleasantness is the result of experimental conditions and methods. Referring to our original study she raises the objection that the subjects "were seated throughout......(the) experiment in a comfortable chair, they were already relaxed, and therefore it was easier for them to be 'passive and receptive' with P than with U stimuli. Their bodily comfort was a source of constant P stimulation." But a more important criticism, she continues, lies in the inadequacy of experimental conditions for producing P responses. "If an *O* has a P stimulus placed under his nostrils, there is no incentive for him to make seeking movements, unless *E* begins to withdraw the stimulus. Indeed, when we repeated the Olfactory Experiment under Young's conditions and instructions, we found no definite seeking movements or tendencies to move."

Corwin, therefore, modified experimental conditions. Instead of making her subjects comfortable in a Morris chair she used one of the ordinary type in which their bodily position would be a matter of indifference. She arranged "a situation in which the *O* must seek if he desires to retain a P." There were three experiments. (1) Vials of P and U smell substances

[1] Young, P. T., Pleasantness and Unpleasantness in Relation to Organic Response, this Journal, 1921, 32, 38f.

[2] Corwin, G. H., The Involuntary Response to Pleasantness, this Journal, 1921, 32, 563f.

were placed upon the end of a moving rod arranged in such a manner that the stimulus would recede from the subject's nostrils at the rate of 1.7 cm. per sec. In order to get graphic records a band, attached to the subject's head, was connected to an ergograph the marker of which wrote upon a smoked kymograph sheet. (2) In a cutaneous experiment P and U stimuli were drawn slowly across the subject's forehead while *E* observed whether or not there were following or avoiding movements. (3) Agreeable and disagreeable music, and other auditory stimuli, were led to the subject's ear through a rubber tube from another room. One *E* controlled the source of sound while another "moved the free end of the tube gradually away from *O*'s ear."

From the introspective reports and the graphic records Corwin concludes "that definitely seeking or maintaining reactions to P stimulations are found in 84.3% of the total number of P cases."

For two reasons it seemed advisable to repeat Corwin's experiment under slightly different conditions. In the first place, her moving stimuli suggested that the pursuit movements which were found may not be genuine expressions of pleasant feeling, and that another interpretation may be possible and perhaps preferable. Corwin herself notes that pursuit sometimes occurs "for another reason than P." In the second place, she has not treated the data comparatively by showing the number of times pursuit was found with U and avoidance with P; and the study is, consequently, one-sided and incomplete. Accordingly we have taken for an experimental problem: movements of pursuit and avoidance in relation to P and U. By 'pursuit' is meant any movement toward the stimulus-object; by 'avoidance', any movement in the contrary direction. Is pursuit, as a matter of fact, correlated with pleasant feeling and avoidance with unpleasant feeling?

I. Experimental

The following experiment was conducted in the Psychological Laboratory, University of Illinois, in the fall of 1921.

The subject[3] was seated in a white enamel, straight-back iron chair. One end of a tape was attached to *S*'s head and the other to a Mosso ergograph which was clamped firmly to a table at the rear. The ergograph recorded upon a smoked kymograph

[3]There were two subjects, Miss H. A. Anderson (*A*) and Dr. C. R. Griffith (*G*), both members of the department.

The chair was selected chiefly to meet the criticism that a Morris chair is relaxing and hence unfavorable to active movements. It is slightly uncomfortable, rather than comfortable. For a brief description see: Griffith, C. R., The Organic Effects of Repeated Bodily Rotation, *Jour. of Exper. Psychol.*, 1920, 3, 20f. and ref.

sheet. After a number of preliminary trials the ergograph was discarded, for it was found to be too insensitive to record very slight involuntary movements. In its place was substituted a Porter[4] lever, which carried a light straw. The lever magnified the actual extent of head movements 7 to 12 times, varying with the straw (which was changed several times during the course of the experiment). As the experiment proceeded the kymograph also was eliminated and graphic records were not taken; it was found that *E* could directly observe and record to better advantage the movements of the pointer. As an aid to observation a white card with black radial lines (about 1 cm. apart, radius 20 cm.) was clamped firmly to a standard behind the pointer. A rubber band, just above the pivot of the lever, kept the tape tight. The position of the pointer upon the scale could be adjusted by moving the standard forward or backward.

The Olfactory Experiment. The following typewritten instruction was used:[5]

"You will be given a series of olfactory stimuli, some of which are *intensive* odors, some have no odors at all. At the signal 'now' smell the stimulus.

"You are asked (1) to report whether the immediate experience is pleasant, unpleasant or indifferent, and to indicate the *intensity of the feeling* (using, for example, such terms as 'very weak,' 'weak,' 'moderate,' 'strong,' 'very strong'); and (2) to report all muscular tendencies and organic sensations in any way related to the affective reaction."

S was seated, his eyes closed. The stimulus was placed beneath his nostrils 1 cm. or more away. He was permitted to take two and, rarely, three full inhalations. The stimulus was not withdrawn except when there was danger of touching *S*, for it seemed methodologically better to hold the vial in such a position that *S* could move freely toward it or away from it, as the case might be, without definitely suggesting movement by withdrawing the stimulus-object. The apparatus was so delicate that it indicated immediately the slightest movement in either direction. *E* learned to watch the indicator in indirect vision.

The following stimuli were used: oil of mace; stale cheese; oil of juniper; anise; ess. wintergreen; camphor; oil of bergamot; oil of lavender; mutton tallow; heliotrope; heliotropine; oil of lemon; tar water; cloves; nitrobenzol; and a series of eight odors prepared by C. H. Stoelting Co. (sulphuric ether, oil of cloves, oil of peppermint, oil of rose, old fish, carbon disulphide, strong cheese and asafoetida).

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[4]The device is a light muscle-lever, with double-hook straw fastener, pictured by W. T. Porter in *An Introduction to Physiology*, 1906, 86. The arrangement proved very satisfactory in that it indicated the slightest movements. Probably these movements could be studied even more minutely by attaching a mirror to *S*'s head, so that a beam of light would be reflected upon the wall or ceiling.

[5]It is identical with the instruction used by Corwin; the last paragraph is practically the same as that of the Minnesota study. *G* asked, somewhat facetiously, how an olfactory stimulus could "have no odor at all!"

To reproduce Corwin's conditions as closely as possible, an electric fan was operated throughout the experimental period. It "served the double function of keeping the air in motion and of eliminating the noise of the kymograph."

TABLE I

Observed movement	P reported Subject		U reported Subject		Indifference Subject		Total
	A	*G*	*A*	*G*	*A*	*G*	
Forward	30	5	2	0	2	4	43
Backward	3	0	23	11	0	1	38
None	4	4	4	2	5	5	24
Forward & backward	1	5	2	5	0	3	16
Total	38	14	31	18	7	13	121

Results. Table 1 shows the gross results. It will be seen immediately that P tends to be associated with forward movement (35 out of 52 cases) and U with backward movement (34 out of 49 cases). Indifference is characterized by no movement (10 out of 20 cases).

With *G* there are three cases of discrepancy between the movements of the pointer as observed and recorded by *E* and the verbal reports of movement processes experienced by *S*. Following are excerpts from these reports:

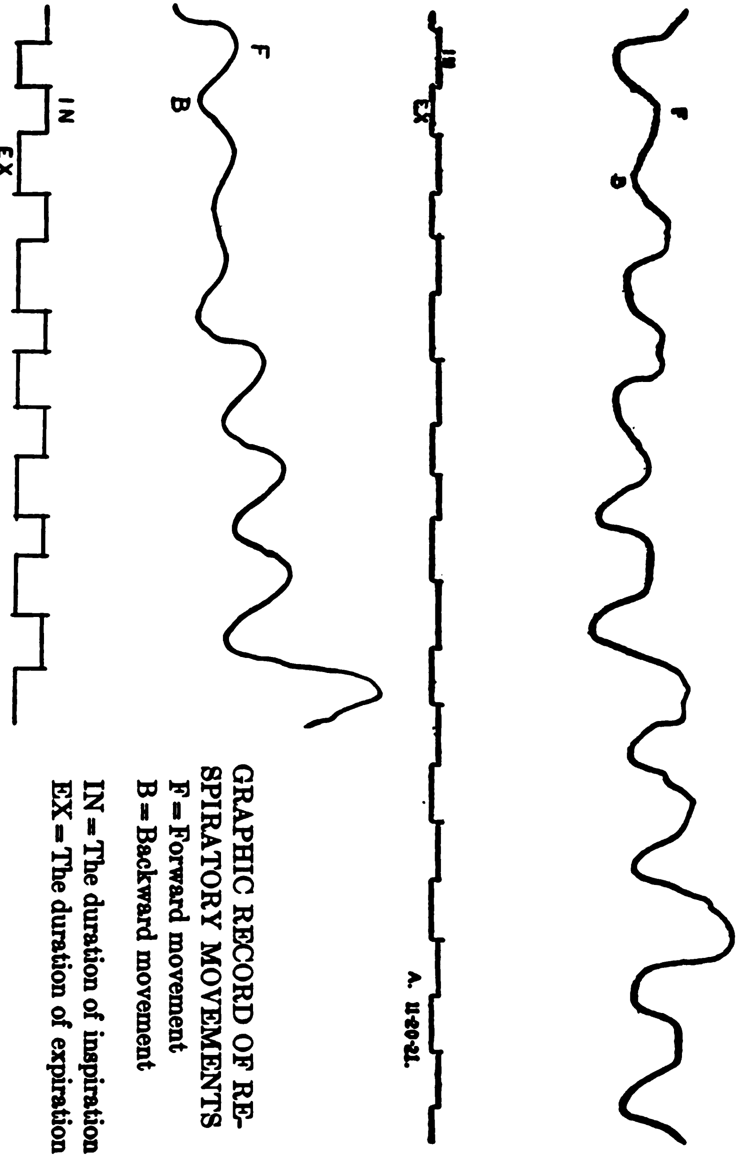
"Decidedly U. Kinaesthesis about mouth and nose and neck as in withdrawal." (Pointer makes a slight movement in the forward direction.) "An exact duplication of the former experience." (The next report; no movement observed.) "Very intense. Extremely U and disgusting. Kinaesthesis about the mouth and face and neck meaning withdrawal." (The pointer makes a gradual and wave-like movement in the forward direction.)

These ambiguous cases have not been included in the data of the table.

The cases in which both forward and backward movements were observed (during the period of stimulation) are usually reports of weak P and U. Following are instances from the data of subject *G*.

"Affectively indifferent." (Slight forward and backward movement.) "Quite P." (Alternate movements in both directions.) "A slight tinge of U." (Movements forward and backward.) "Neither P nor U affectively." (Movements in both directions.) "Faintly U to start with; becoming I as the olfactory quality disappeared." (Slight forward, then backward, then forward movement observed.) "Faintly P." (Forward, backward, forward.) "Rather I." (Forward, then backward movement.) "Very faint; just perceptibly P." (Forward, then backward movement.) "Mildly U becoming very U toward the end." (The pointer makes a slight, gradual forward movement and then a backward.)

One report of *A* was thrown out because it was too complex affectively; the movements of the pointer were complicated and could not be adequately observed by *E*.



GRAPHIC RECORD OF RESPIRATORY MOVEMENTS
F = Forward movement
B = Backward movement
IN = The duration of inspiration
EX = The duration of expiration

From the quantitative data and from the reports of the subjects two conclusions may be drawn. (1) There is probably a tendency for forward movements to occur when *S* reports pleasantness, and also a tendency for backward movements to be associated with unpleasantness. Indifference and weak P and U are characterized by no movement, or else by slight movements in both directions. (2) It is apparent that the correlation is not perfect; in this respect it resembles other correlations gained by the expressive methods.

Breathing Movements. Since every case of smelling is a reaction in which air is inhaled through the nostrils and into the lungs, it is important to know whether normal breathing is accompanied by any characteristic forward or backward movement. To test the matter a number of graphic records were taken on both subjects and also on two individuals not regular subjects in the experiment.

S was instructed to breathe deeply during the test; his eyes were closed. *E* stood to the right of *S* and slightly in front; from this position he could readily observe respiration. At the beginning of every inspiration *E* pressed a hand-key, keeping it pressed throughout the inhalation period; at the beginning of expiration *E* released the key, keeping it released during the period of exhalation. The key was connected to an electric marker which left a tracing upon the smoked paper. The kymograph was not started until breathing seemed normal and regular.

The curves show that inhalation is accompanied by a slight backward movement of the head while expiration is accompanied by a slight forward movement. The changes are purely mechanical and "express" the fact that breathing involves in a particular way the musculature of the chest, shoulders and neck.

For present purposes the significance of these respiratory movements is this: inspiration is accompanied by slight "withdrawing" movements which bear no special relation to the affective phase of experience; and, similarly, mere exhalation is accompanied by "seeking movements." Respiration may account for some of the backward movements associated with affectively weak or indifferent stimuli; and possibly, also, for those instances in which both forward and backward movements were observed; but in these cases the forward movement was regularly observed before the backward, indicating that there are other complicating factors involved.

Pursuit Movements and Attention. (*A*) *Olfactory Reports.* Both subjects report that when the olfactory quality is of weak intensity there are movements toward it in order to get a better smell, a more intensive and a clearer quality. Possibly sensory

adaptation which rapidly reduces the intensity of weak olfactory processes gives rise to "seeking movements" of this sort. At any rate these movements "express" olfactory attention and have at best a doubtful relation to the affective phase of the experience. Following are excerpts to illustrate the point:

Subject A. "P. A tendency toward it; I think that was because it was weak." "P. A slight tendency to go in the direction of the object, but that was because it was weak as much as anything else." "P. A tendency to draw toward it, largely because it is weak." "P. A tendency to draw towards it, partly because it is weak."

Subject G. "Faintly P. Just a little kinaesthesis about the face as if trying to smell to better advantage." "Rather indifferent. A little kinaesthesis about the neck as if leaning forward trying to get a better perception." "A faint tinge of U holding over from the last experience. A bit of kinaesthesis as if trying to get a better smell." "No olfactory quality. The whole experience was indifferent affectively. There may have been a slight change in pressure meaning bending forward to get a better smell." "Rather indifferent. Leaning forward to get a better smell." "Affectively indifferent. A change in kinaesthesis which meant leaning forward to get a better smell." "Perhaps a shade of kinaesthesis which meant leaning forward to get a better smell. Nothing affectively."

In this connection it should be noted that *G* sometimes refers to an 'inviting' character which is not identified with P or U. "It was not P or U; it was rather inviting." "There was a piercing quality that was inviting." "A faint tinge of something rather curiously inviting. I don't know whether there was any positive kinaesthesis of approach or whether it was just the absence of withdrawal." (Slight forward movement recorded.) "Curiously P; rather invitingly P." "Rather invitingly P. The inviting part is carried in kinaesthetic tendencies to move forward. Later it became faintly U and the kinaesthesis that had meant forward movement then dropped out." "Neither P nor U; affectively neutral. Rather inviting; carried in terms of kinaesthesis and those muscular tendencies and the bodily set necessary to get a better smell."

This 'inviting' character is apparently related to approach and the attempt to get a clear perception, but it is not to be confused with P.

(*B*) *The Auditory Experiment.* In view of the result that sensory attention may be accompanied with slight forward movement it seemed advisable to study the involuntary response to an affectively indifferent auditory stimulus. Corwin led phonograph music to the ear of *S* through a rubber tube and then gradually withdrew the tube from the ear. In this way evidence of "seeking" movements was produced; but some of the reports, especially those of *Bi*, indicate that an attentive factor was involved. The present problem is this: what is the normal response to an indifferent auditory stimulus the intensity of which is gradually and continuously decreasing?

A watch was used for stimulation. The following instruction explains the experimental procedure:

"After a 'ready' signal the experimenter will hold a watch near to your ear, and then move it gradually away. Attend to the sounds.

"You are instructed to describe the experience, noting especially kinaesthetic and organic processes."

The subject's chair was turned so that the apparatus lay on a table at the side. The tape was placed around *S*'s head and attached to the indicator. The slightest movement of *S* was immediately apparent to *E*. During the trials *S* sat with eyes closed. Twenty observations were made with each *S*: in the first series, five *R* and then five *L*; in the second, five *L* followed by five *R*. *E*, keeping his eye upon the seconds-hand of the watch, gave the preparatory signals and then placed the watch near the ear of *S*. It was moved away gradually for a distance of about 1 m. and over a period of time which varied from 12 to 25 sec. *E* noted carefully the movements of the pointer and recorded the result as well as the reports of *S*.

In every case, without a single exception for both subjects, there were definite movements toward the stimulus; and the order of magnitude of these movements is the same as that in the olfactory experiment. The movements were found for both ears. The verbal reports confirm the observations of *E*.

A reports as follows. "As the sound became fainter and fainter there was a concentration of attention to it. A tendency to grit the teeth and a tendency to move in the direction of the sound. This seemed a desire to do so rather than a tendency. There was a general tenseness of the body as it became fainter; relaxation when it seemed to get closer and louder." "Gritting of the teeth; closing of the eyes; muscular contraction; tendency to move in the direction of the sound." "Increased tenseness of the body as the sound gets fainter. When it appears again there is relaxation. A tendency (slight and perhaps imaginary) to move in the direction of the sound." "Tenseness about the shoulders, neck, and a slight tendency to move in the direction of the sound." "Turning of the eyes in the direction of the sound. Visual and kinaesthetic imagery of turning the head." "Slight strain sensations about the neck, shoulders, hands. Tendency to hold the breath.... A tendency to move in the direction of the sound." "A tendency to move in the direction of the sound; it seemed to be inhibited. A tendency to turn the eyes in the direction of the sound." "A decided tendency to move toward the sound—mostly inhibited. Holding of the breath. The eyes turned toward the sound." "A slight tendency to move toward the sound. There was some strain sensation."

G did not report movement toward the stimulus-object. At the close of the series he was asked whether or not he was aware of any movements. He replied: "Once in a while it seemed as if the kinaesthesis in the neck might be the result of movement but I am not sure. Once I thought that tactual processes from my collar meant movement but again I wasn't sure. These processes are part of the general set to get in the best attitude for hearing. I think there may have been a little movement."

Two types of forward movement were observed. (1) There was a gradual, progressive settling toward the source of stimulation—to the right or left—which continued through the series. This type of movement was indicated by the necessity of readjusting the standard which supported the indicator. The standard had to be moved away from *S* constantly so that the pointer would come to a vertical position before each test. The series of experiments was not long enough to determine how far this gradual and continued "seeking" would

go before coming to an equilibrium.[6] (2) There was an immediate and definite forward movement which commenced shortly after the stimulus was presented and which continued throughout the relatively brief period of stimulation.

II. Discussion of Results

Pursuit and Pleasantness. The experiments under auditory stimulation show conclusively that if *S* merely attends to weak sounds, or to sounds which are becoming intensively weaker, there are movements of approach which indicate sensory attention. These movements, observed when *S* is affectively indifferent, are of the same order of magnitude as those found in the olfactory experiment with P and U odors. Now if pleasant feeling is normally associated with attentive response and unpleasant feeling is accompanied by inhibition of the normal reaction, or by avoidance, or by some other response which may be called inattentive, then attentional pursuit should correlate, in the long run, with P, and its absence or opposite with U. It is extremely doubtful whether the pursuit movements observed in the present experiment express anything more than sensory attention. The actual extent of the movements of *S*'s head is very slight,—usually less than .5 cm. and never more than 2 cm. at the very most. Certainly such movements cannot be interpreted directly and unequivocally as expressions of pleasant feeling.[7]

The failure to find satisfactory evidence of seeking movements which are involuntary expressions of pleasantness is due once again, presumably, to experimental conditions. Corwin stresses the point that there must be an "incentive" to seeking; and her experimental conditions provide adequate incentives. In both the original experiment and the present one this element was lacking. *S*, indeed, was entirely free to move in either direction, but there was no suggestion of movement, unless possibly it were the tape attached to *S*'s head. Had there been some incentive, it is probable that gross seekings would have appeared. Corwin herself states that when she repeated the original experiment under my conditions, *i. e.*, without incentives to seeking, she found no evidence of "definite seeking movements or tendencies to move." If, therefore, some in-

[6] I believe that the same sort of gradual trend can be made out in the experiment with olfactory stimuli. Some of the respiratory curves, also, indicate a gradual adjustment in the forward direction.

[7] This result suggests the conclusion of H. C. Stevens that the "method of expression has failed in the domain of feelings" because of "complications with other mental processes, and....the psychophysical processes of the sensation." Stevens, H. C., A Plethysmographic Study of Attention, this Journal, 1905, 16, 470.

centive is necessary for the appearance of gross seeking movements, we may well ask: in what sense are gross seeking movements an expression of pleasant feeling?

Comparison of Experiments. A comparison of my two experiments may possibly throw light upon the nature of seeking movements.

Under the original conditions[8] there were 31 cases of P reported as having strong intensity against 27 cases of intense U: under present conditions there are only 3 cases of strong P against 25 strong U. In the original experiment, again, there were reported as many cases of relaxation as of strain:[9] in the present affective experiment (with olfactory stimuli) relaxations are decidedly lacking. *A* reports 3 cases (all in P reports; not one, however, in the *intense* P reports); but *G* does not mention a single case. On the other hand, there is relatively much more straining reported by the *S*s in the present data, *A* alone reporting 21 cases of strain associated with U. A great deal of the straining can be referred definitely to attentive attitudes.[10] Although there are dangers in comparing two sets of data gained under different conditions, nevertheless the following statement is completely warranted by the facts; *viz.*, that under the original conditions there was passivity, relaxation, and moderately intense P feeling, relatively more intense P than U; and under present conditions there is a more active bodily attitude, little or no relaxation, but much strain (especially attentive), and a greatly reduced number of intense P reports relative to the number of intense U. It should be remembered that the present experimental situation, with the iron chair and *S* harnessed to an apparatus, was devised specifically to meet the criticism that the original conditions favored relaxation and passivity and that on this account they were unfavorable to active seeking movements. It turns out (1) that present conditions which presumably favor an active type of response by making bodily relaxation next to impossible are not at all favorable to the development of affectively intense pleasant feelings[11] and (2) that the failure to find valid evidence for pursuit movements as expressions of P is not due to *S*'s comfort or lack of comfort but rather, as Corwin herself remarks, to the lack of a special incentive.

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[8]*Op. cit.*, 46.

[9]*Op. cit.*, 40.

[10]It should be noted that *A* reports her strains as contractions and her relaxations as expansions or openings (of the throat). *G* speaks of "muscular contractions" and describes as "kinaesthetic sensations" processes which other *S*s might describe as strain. The distinction between relaxation and expansion, made by Corwin's observers, *may be* valid, but at the present time I am inclined to think that it is largely verbal.

[11]The fact that relaxation favors the development of P feeling does not

III. Critical

Incentives and Seeking Movements. **Common instances show that seeking movements are not expressions of pleasant feeling** but rather of some incentive or need. (1) Imagine *S* seated comfortably in a Morris chair listening to his favorite piece of music played upon a phonograph. If the selection is loud and clear, he will probably lean back and relax (not a withdrawing movement, however, merely because the phonograph happens to be in front!); he may beat time. In any case the affective experience is ordinarily P. If the music be intensively weak so that *S* would have to 'strain' to hear it, or if *S* had to pursue a receding rubber tube in order to hear the music well, the total affective experience would not be so P as under the first conditions. Hence seeking movements do not express P feeling *as such*; they show, rather, that the conditions for P are not wholly satisfactory in some respect or other. *S* will make movements which are necessary to gain satisfactory conditions or to maintain them, if need be; but the seeking movements are not involuntary expressions of P. (2) In definite and extreme cases of seeking behavior the affective experience is normally U. A drowning rat "seeks" air: a hungry dog "seeks" food: an animal in heat, confined in a cage, "seeks" a mate: a man in a burning house "seeks" safety. These seeking movements express the presence of some need, irritant or incentive rather than P or U; but, if we are bound to associate seeking movements with either of the traditional affective qualities, I submit that definite and extreme seeking behavior is ordinarily accompanied by U. (3) There are also instances of apparent "seeking" which are affectively indifferent. Following an insect or a moving object from curiosity is a case in point. The entire group of attentional "seekings," referred to above, should be mentioned here. (4) Again there are complex and prolonged examples of seeking which involve many changes from P to U or I. Consider a hunting expedition in which a day is spent hunting prey; or consider a life-time spent in seeking a fortune or fame. In these cases there is some

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mean that all Ps are essentially or even typically relaxations. There are other types not dealt with in the present series of studies.

There can be no doubt that P is associated with active bodily response in such instances as dancing, beating time to music, free swinging movements (as in certain games), bodily exercises, etc. It also appears that normal uninterrupted physiological functions are frequently, if not normally, accompanied with pleasant feeling. Obvious examples are deglutition and the habituated manipulations which accompany it, coitus, even defecation and urination in some cases, and the entire group of free movements referred to above. In an unpublished experiment upon the affective phase of active movement, two subjects report P associated with free swinging movements of the leg, flexed at the knee.

determination (motive, incentive) which expresses itself in seeking movements, but any attempt to relate the movements themselves to either of the traditional affective qualities is utterly futile.

There can be no doubt that "seeking" movements *may be* associated with P. Corwin's experiment proves the point; but the conditions for such an association involve two factors: (*a*) a weak or receding stimulus, or some other unsatisfactory element in the conditions—a need, irritant, incentive, motive—which makes it necessary for *S* to seek or pursue if he is to react adequately and to maintain conditions under (*b*) stimulation which is normally P. The factors which condition seeking movements are obviously neither necessary nor favorable to the existence of P feeling. The necessity for seeking reduces the intensity of P obtainable and the reduction may go to I or even to U; extreme cases of seeking are U rather than P. If seeking movements express anything, that is an incentive, need, or irritant rather than simple feeling.

It is impossible, therefore, to accept Corwin's interpretation that "the direct response of the organism to P is....a definite activity of pursuit or of tendencies to pursuit." Obviously she has got out of her experiments exactly what she put in. With a little thought it would be possible to devise experimental conditions under which pleasantness would appear to be associated with movements of avoidance.

Sources of Confusion. There is yet another serious objection to the traditional hedonistic doctrine. The reputed correlations between P and seeking and between U and avoidance are both teleological interpretations of behavior. "Seeking" implies an organism purposefully striving for some object or situation which is the aim in view; "avoiding" may have a similar connotation. The teleological nature of these terms is obvious from the following illustrations. Relaxing in a Morris chair while listening to music is "withdrawing" from the phonograph and "seeking" the chair, according to the way in which we interpret the situation. A hunter goes forth to seek a bear; but when the animal is found, perhaps the same man "seeks" safety by "avoiding" the bear. The random incoordinated movements of a drowning rat may be interpreted as "seeking" air or "avoiding" confining conditions. One "seeks" food to "avoid" hunger. Illustrations of this sort could be increased indefinitely to show that "seeking" and "avoiding" are teleological and, moreover, dynamistic interpretations of behavior.[12] The statement that behavior has some purpose or other does not

[12]For the present study these terms were defined in a purely descriptive way to avoid the above implications.

contribute anything to an exact analysis of the facts; it is at best a preliminary statement. Since one can easily read into the same response either the purpose of "seeking" or the purpose of "avoiding," the teleological interpretations lead only to confusion of thought; they do not make for logical clearness.

But there is another source of confusion. Those moralists and biologists who associate P feeling and seeking movements have fallen into an error which I have called 'the meaning error'.[13] To say that one seeks pleasure is not the same as to say that seeking is a bodily accompaniment of P, a mark or expression of pleasantness which is truly felt. What is meant is that one seeks the conditions of pleasure, or seeks to maintain them after they have been obtained. This statement, whether true or false, has little to do with the factual question raised in the present experiment. It may be true, in a sense, that the readiness to seek is the roughest kind of indication of those objects and situations capable of pleasing us; but this is very different from regarding movements of search as typical marks, or expressions, or involuntary characteristics of felt pleasantness. There is no good evidence for the latter belief.

Avoidance and Unpleasantness. It should be noted that, in the case of U and avoidance, there is a solid body of fact which lends itself readily to the traditional interpretation. Characterizing the U response, I have previously noted[14] the "well-known tendency to withdraw oneself from the stimulus-object, either reflexly or deliberately.... (and) the tendency to put the object away from oneself or to prevent its action.... (and the) tendency to inhibit or resist....some normal response to the stimulation...(and finally, the presence of) reflex twitchings, convulsive contractions, and bodily reverberations of various sorts, especially when the stimulus-object is presented suddenly and there is an element of surprise." Neglecting the last mentioned characteristic, it appears that the others have the biological meaning of avoidance, defence or resistance.

The present study, however, indicates that nothing is added to the facts by interpreting these U responses as biological avoidances. As in the case of P and pursuit, there are obvious exceptions to the reputed correlation.[15] It cannot be maintained that all avoiding expresses U. Outstanding instances of the relationship (jerking away the hand when burned, turning the head from a disagreeable sight, holding the breath or vomiting when an U odor is presented, etc.) do not justify a sweeping generalization.

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[13]Young, P. T., An Experimental Study of Mixed Feelings, this JOURNAL, 1918, 29, 261f.

[14]*Op. cit.*, 49f.

[15]Suppose, for example, that we define avoidance as getting away from

The direct explanation of "seeking" and "avoiding" behavior may be given ultimately in physiological or chemical terms, but it should be remembered that the mechanisms of response are extremely complex. The central nervous system is highly variable and a thousand factors may intervene between stimulus and response. One factor alone—the experimental instruction, or the self-instruction, of *S*—is capable of turning a "seeking" movement into an "avoidance." Consequently it seems methodologically better to await more information than to read into the facts now at hand some purpose.

Summary

1. In the case of olfactory stimuli of moderate intensity pleasant feeling is sometimes associated with a slight forward movement of the head; and unpleasant feeling, with a slight backward movement. Occasionally no movement at all, or both forward and backward movement, is observed. The reports indicate that forward movement is frequently an indication of olfactory attention. Any interpretation of the data is complicated by the fact that inhalation is accompanied by a backward movement and exhalation by a forward; these respiratory movements are purely mechanical incidents in deep breathing.

2. Attention to a weak and affectively indifferent auditory stimulus, or to one which is decreasing in intensity, is indicated by an involuntary muscular adjustment toward the stimulus. Two types of movement have been noted: (1) a gradual trend toward the source of stimulation which continues throughout the experimental series, and (2) brief and relatively marked movements during the period under controlled observation.

3. Seeking movements, other than those of attention, do not appear as "expressions" of pleasantness unless some incentive is introduced into the conditions. The incentive probably reduces the intensity of feeling obtainable and is not

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an object or situation, or casting it aside. Immediately one thinks of taking off an overcoat and hat. Such behavior has no apparent relation either to P or to U, although it is definitely a matter of casting aside. The next illustration that occurs is a chase in which one animal runs away from another. Are we to assume that the avoiding animal feels only U and that the pursuing animal experiences nothing but P? Again, Professor Bentley has called my attention to the fact that the play of courtship, which may be assumed to be P, is generally regarded as pursuit on the part of the male and as avoidance on the part of the female.

It may not be far-fetched, accepting the above definition, to regard urination and defecation as instances of rejection; and these experiences, according to Boring's observers, may be P, U, or I, varying with the individual. Boring, E. G., Processes Referred to the Alimentary and Urinary Tracts: A Qualitative Analysis, *Psychol. Rev.*, 1915, *22*, 323f.

necessary to the existence of pleasantness. Consequently the statement that pleasantness is expressed by involuntary seeking movements is abandoned.

4. A critical analysis shows that seeking movements indicate what might be called a need, irritant, incentive, or determination, rather than simple feeling, and that "seeking" and "avoiding" are teleological interpretations of behavior, and, further, that the reputed correlations involve a confusion between meaning and affective processes. For these reasons it is recommended that the traditional hedonistic doctrine be abandoned and that psychologists study directly the facts of response and of affective psychology.

DIOTIC TONAL VOLUMES AS A FUNCTION OF DIFFERENCE OF PHASE[1]

By H. M. Halverson, University of Maine

The fact that a tone, diotically perceived in the presence of a phase-difference between the two ears, is localized toward the side of the leading phase,[2] suggests that the two stimuli, in integrating to form a single tonal sensation, contribute each a tendency to localization toward their respective sides, and that the stimulus leading in phase, because of its priority, contributes to the spatial aspect of the integration more than does the stimulus that lags. When there is no phase-difference localization is in the median plane: both stimuli contribute equally. When one stimulus leads by half a wave-length localization may be entirely to one side: it would seem as if the tone of prior phase were inhibiting the other tone. To such facts the Bernstein theory of localization with the modifications suggested by Boring[3] seems applicable.[4] On the modified Bernstein hypothesis we should expect median localizations, where displaced excitations unite in a single larger region of excitation, to show a large tonal volume (provided, of course, that it should prove to be correct to equate tonal volume to the analogous attribute of spatial diffusion for the skin); and we should expect lateral localizations, where one stimulus inhibiting the other is singly effective, to show a small volume. The present experiment was undertaken in order to test this hypothesis. The results do not substantiate the hypothesis in any simple manner, since the medianly and laterally localized tones are largest and the intermediate tones smallest. The results are, however, positive and univocal, and are presented here without attempt at a theoretical explanation, but with the conviction that an ultimate theory of auditory localization will have to take account of them.

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[1]From the Psychological Laboratory of Clark University.

[2]For the writer's recent papers on this topic and references to the literature, see H. M. Halverson, *Psychol. Monog.*, 1922, no. 140, 7-29; this Journal, 1922, 33, 178-212.

[3]E. G. Boring, *Quart. J. Exp. Physiol.*, 1916, 10, 86-94.

[4]Watt also makes use of the Bernstein theory in his theory of hearing, but in a different manner; H. J. Watt, *Psychology of Sound*, 1917, 175-192; *Brit. J. Psychol.*, 1920, 11, 163-171.

The apparatus was the same as the apparatus for localization with closed tubes described in a previous paper.[5] The source of tone is a 512 d. v. tuning fork, electrically driven by a master-fork. The tone is conducted to the two ears by a pair of rubber tubes which pass through a wall. The phase-relation of the tones of the two tubes is altered by means of a telescopic slide in one of the tubes, which lengthens or shortens the path.

The observers were Mr. M. A. Tinker (*T*), Mr. M. K. Macdonald (*M*), and Mr. F. L. Bixby (*B*), all members of the Psychological Laboratory of Clark University. *T* and *M* were graduate students and trained observers; *B* was a senior of considerable laboratory experience. None of the *Os*, however, had had any previous experience in experimental auditory localization. Their inexperience in localization was considered an advantage in this problem, since the attention was to be to the volumic aspects of the experience and away from its localization.

In order to train the *Os* in volumic judgments and to make sure that their definition of volume had the accepted meaning, a preliminary experiment, patterned after that of Rich[6], was instituted. The *Os* were required to make volumic judgments under the method of constant stimuli upon tones from two Stern variators. The standard was 396 d. v., and the five comparison stimuli ranged from 1/8 to 5/8 of a musical tone lower than the standard. At first the *Os* failed to judge consistently. By the end of the second hour, however, *T* and *B* were giving constant results, and by the end of the fourth hour *M* was equally constant. The judgments were not immediate at first, but became immediate as soon as the *Os* had been required to characterize "volume" in a written introspective report. The psychophysical results agree very closely indeed with those of Rich. The average limen for these three *Os* at 396 d. v. is 14.3 d. v. The average limen for Rich's two *Os* at 400 d. v. is 13.4 d. v. Rich's lower limens obtained with tuning-forks[7] are not comparable, since these experiments, like Rich's earlier trials, were with the Stern bottles. The general agreement of these results with Rich's may, however, be taken as an assurance that the *Os* had learned to make judgments of volume as ordinarily defined.

With the *Os* trained to appreciate volumic differences, it was possible to proceed to a preliminary investigation of the volumic relationships of diotic tones where the phase-relationship is varied. Table I gives the results of this preliminary

[5] Halverson, this JOURNAL, 33, 180f.

[6] G. J. Rich, *J. Exp. Psychol.*, 1916, 1, 13-22.

[7] Rich, this JOURNAL, 1919, 30, 149-153.

TABLE I

Preliminary comparison of volumes of diotic tones (512 d. v.; wave-length = 67.3 cm.) for phase-differences of 11.5 cm. (localized 30° left), 24.8 cm. (60° left), and 35 cm. (90° left), compared with zero phase-difference (median localization) as the standard. Observers *T*, *M*, and *B*. Figures are percentages based on 50 judgments for each comparison stimulus.

Localization	30° left			60° left			90° left		
Phase-difference	11.5 cm.			24.8 cm.			35.0 cm.		
Observer	Larger	Equal	Smaller	Larger	Equal	Smaller	Larger	Equal	Smaller
T	0	14	86	0	24	76	16	64	20
M	8	30	62	0	38	62	30	52	18
B	0	32	68	16	34	50	26	56	18
Average	3	25	72	5	32	63	24	57	19

survey. The three phase-differences which give on the average 30°, 60° and 90° left localization were selected for comparison with zero phase-difference, which gives median localization. The table shows that the difference in volume between each of these three comparison stimuli and the standard is not so great as to give univocal judgments. In all three cases the two tones are frequently called "equal," and there are some judgments in the category of difference opposed to the predominant category. Subsequent results (Table II) show, however, that after practice this degree of scatter is reduced. In general it is apparent from Table I that the tone localized at 30° left is predominantly smaller than the medianly localized tone, and that the tone for 60° left is also predominantly smaller, although perhaps not quite so small. For the tone localized laterally at the left, the "equal" judgments predominate; the judgments of "larger" outnumber the judgments of "smaller" for two *O*s, whereas for one *O* the balance is the other way. The lateral tone would thus seem to be of a volume about equal to the volume of the median tone.

The next step is to determine the exact course of the volumic changes, indicated in the preliminary experiment, as phase-difference is altered from zero to the maximum of about a half wave-length, the difference where the tone is localized at 90° left and is about to shift abruptly to 90° right; for a tone leading in phase by more than one-half wave-length is functionally to be regarded as a tone lagging by less than a half wave-length.

The method of procedure was to select zero phase-difference as a standard, and then to determine by the method of constant stimuli the limen for the category "smaller." The

TABLE II

Psychometric functions of volumic differences for phase-differences of diotically perceived tones of 512 d.v. Relative frequencies based on 50 judgments for each comparison stimulus.

Series & standard stimulus (cm.)	Obs.	Range of 5 equally separated comparison stimuli (cm.)	Relative frequencies of volumic difference. I, II, III, "smaller;" IV, V, VI, "larger." Successive comparison stimuli in increasing order of phase-difference 1	2	3	4	5	Limen by linear inter-polation (cm.)	Average limen (cm.)
I	*T*	6.0–9.0	20	32	52	60	80	7.4	
0.0	*M*	4.0–10.0	20	40	60	74	88	6.3	7.0
	B	5.0–8.0	22	36	44	50	72	7.2	
II	*T*	8.0–12.0	16	38	52	56	68	9.8	
7.0	*M*	8.0–10.0	24	40	66	60	78	8.7	9.2
	B	8.0–10.0	24	24	44	60	78	9.2	
III	*T*	10.0–12.0	28	34	38	52	40	11.5[1]	
9.2	*M*	10.0–12.0	28	38	52	40	52	11.8[2]	11.6
	B	10.0–12.0	18	32	40	46	62	11.6	
IV	*T*	18.0–30.0	10	30	32	62	74	25.8	
11.6	*M*	16.0–24.0	22	36	40	46	68	22.4	24.5
	B	22.0–27.0	20	26	40	56	66	25.3	
V	*T*	28.0–32.0	22	40	52	66	84	29.8	
24.5	*M*	25.5–31.5	26	38	54	70	94	28.1	29.7
	B	27.0–32.0	14	26	34	42	62	31.2	
VI	*T*	31.0–35.0	20	18	28	44	42	[3]	
29.7	*M*	31.0–35.0	24	36	40	50	54	34.0	
	B	33.0–35.0	18	24	30	32	44	[3]	

[1]Minimum of psychometric function from parabola through points 3, 4, and 5. Limen equivocal.
[2]Minimum of psychometric function from least-square parabola through all five points. Limen equivocal.
[3]Limen indeterminate; psychometric function does not include 50%.

Os were allowed to report "smaller," "equal," or "larger," but of course no limen for "larger" could be obtained since variation of the stimulus in either direction from the standard tended to give "smaller." When the first set of limens had been obtained (Series I), the limens were averaged for the three *Os*, and this average was used as a new standard for the determination of a new set of limens (Series II). In this manner it was intended to step off the total volumic range by liminal steps. The limit of volumic increase was, however, reached in the third set of limens (Series III). Here the psychometric functions for the *Os* could not be brought to high relative frequencies of the category "smaller;" the minimum volume had been reached, and the functions were reversing direction. Indeed the psychometric functions only barely crossed the 50% abscissa in the cases of *T* and *M*, and returning across it left the exact value of a limen equivocal. For this reason in the column of limens in Table II minima instead of limens are given for these two *Os*. The minimum for *T* was found by passing a parabola through the last three points, and that for *M* by adjusting a parabola by the method of least squares to the entire five points.

Since Series III brought *E* to the volumic minimum, it was necessary for him in Series IV to proceed with limens for the category "larger," just as he had to work only with the category "smaller" when at the maximum in Series I. Series IV was taken with the average of the two minima and one limen of Series III as a standard. Series V was based on the average limen of Series IV, and Series VI on the average limen of Series V. In Series VI maxima were reached, not in the sense that the function again reversed direction, but because the tone now tended to shift to the other side of the head, sometimes existing on both sides at once. This complication would have made careful judgment impossible; moreover it indicated that the limit of variation had been reached, since the tones of the right quadrant simply followed the law of the left. Since the maxima for *T* and *B* in Series VI lay below 50% it is not possible to compute limens for them.

The limens of Table II are all computed by linear interpolation between the two relative frequencies adjacent to 50%. It is obvious from a study of the psychometric functions of Series III and VI that the *phi-gamma* hypothesis is not applicable in those cases. It is also a question whether it should be applied in Series I and IV, which also are limiting cases. Moreover, it is questionable whether any general hypothesis, in the face of the insufficiency of knowledge of the function concerned, could have much value. An hypothesis seeks to take into account remote points as well as the adjacent ones in the

determination of the limen, but it depends for any general validity upon the comparability of different regions of the abscissa scale. In the present case the change of volume for change of phase is irregular; for stretches of the abscissa it seems to change scarcely at all, and then an abrupt change appears. For this reason the mere selection of equal increments of phase-difference for the comparison stimuli gives no assurance that the stimuli are equal in a psychophysical sense and that the remote points can be used to determine the limen. There is no perfectly valid way out of the difficulty. The observed fact, assuming continuity of function, is that the limen lies between the stimulus values for the two relative frequencies that include 50%. If one seeks to say just where, one may follow the convention of linear interpolation as readily as any more difficult assumption. As a matter of fact, with unusual psychometric functions of this sort, the limen has very little meaning, and attention has to be paid to the form and nature of the entire function.

In Fig. 1 the data of Table II are shown graphically in such a way as to indicate the general form of the volumic function. Against difference of phase are plotted for each *O* the six psychometric functions of the six successive series. The ordinate scale for each psychometric function is so arranged that its zero comes opposite the limen of the preceding psychometric function. The average limen for any series was taken as the standard for the succeeding series, and the zero for the scale may be placed at the frequency that the standard had in the preceding series, on the assumption that the standard in the new series would give 0% difference with itself (a rough but not perfect assumption, since a time-error may exist). In this manner it is possible to plot off the volumic function with liminal steps as the ordinate unit and roughly to fill in the breaks between the six observed liminal points by plotting the psychometric functions for these points. The total constellation of the grouped psychometric functions of Fig. 1 gives as closely as possible on the basis of these results the form and amount of the volumic function in general.

Inspection of the three charts of Fig. 1 shows striking agreement among *O*s. It is possible to state the form of the function in general for all *O*s.

As phase-difference increases from zero, the decrease in volume of the diotic tone is at first gradual; and then from about $\pi/8$ to $3\pi/8$ there is an abrupt and rapid decrease to the minimum volume, which is about three liminal steps smaller than the volume for the tones in phase. After $3\pi/8$ there appears to be a gradual increase up to a phase-difference of about $3\pi/4$; thereafter the increase in volume is rapid although not

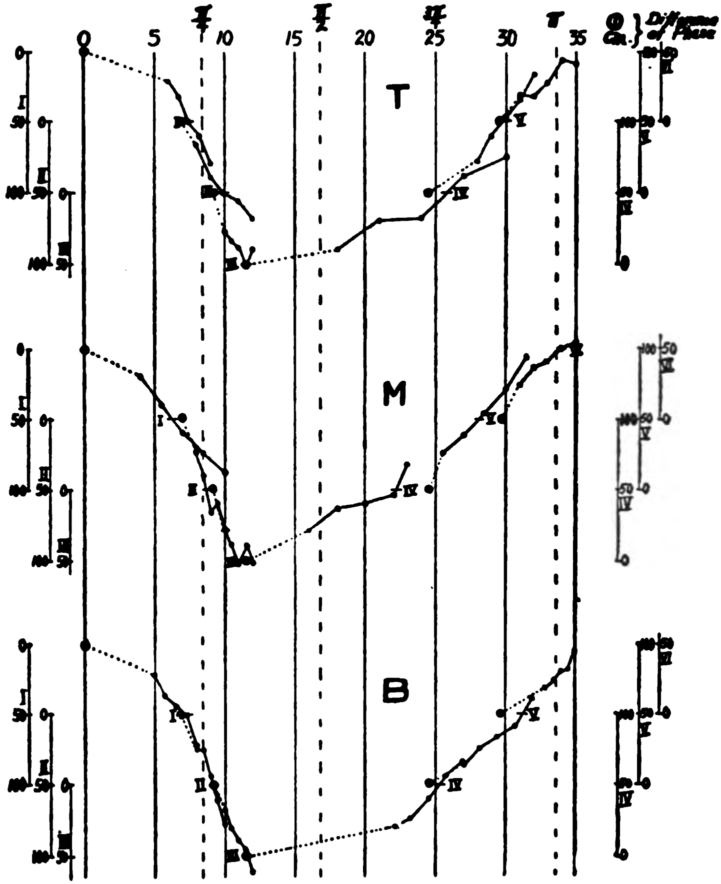

Fig. 1. Volume of diotically perceived tones as a function of difference of phase. Observers *T*, *M*, and *B*. Tones of 512 d. v.; wave-length = 67.3 cm. Abscissa is phase-difference of tones between the two ears, in cm. and wave-length. The psychometric functions for smaller volumes (Series I, II, and III), and for larger volumes (Series IV, V, and VI) are shown successively so scaled that 0% for one function corresponds to the limen of the preceding function, for the reason that the average limen of one function was always used as the standard for the next. For this reason the dotted lines are added on the rough assumption that the standard would give 0% difference from itself as comparison stimulus. The total constellation of the psychometric functions for each *O* gives approximately the form of the function that volume, measured by liminal units (ordinates), is of phase-difference (abscissa).

so rapid as the preceding decrease. The volume reaches a maximum at π where it about equals the volume for zero difference of phase. This last result of Fig. 1 accords with the preliminary results of Table I, which also indicated that the median and laterally localized tones are of about the same volume.

Some *Os* in the localization of diotic tones have described the image of localization as getting farther away from the head when it passes from the median position of localization, and then coming in close to the head when the position of lateral localization is being reached. This change of distance in the image accords with the volumic function; the smaller volumes, it would seem, are perceived as further distant.

The question still remains whether these volumic functions truly represent a distinct volumic aspect of the tonal sensation. There are two other obvious possibilities; the judgments might be dependent entirely or in part upon pitch-difference or upon intensive differences.

The most direct source of information is the *Os'* reports. They consistently described the tones as spatial and their judgments as indicating the spatial aspect. For them the tones are "full," "fat," "lean," "tridimensional," "large," "small," "homogeneously filled," "big," "thin," "extensive," "crowding in from all sides."

The well known association of volumic change with change of pitch and the fact that these *Os* were trained to observe change of volume with change of pitch in the practice-series may nevertheless raise the question whether pitch-differences could be present here and lie at the basis of the volumic function. It is difficult, however, to see how an effective difference of pitch could occur. The lengthening of the one tube with respect to the other might lower the pitch at one side. In such a case there should be binaural beats, but no beats were heard. Moreover a continuous increase of friction would not account for the reversal of the change in the volumic function. It seemed important, nevertheless, to test the matter directly, and the *Os* were asked to make discriminations of pitch between the diotic tone of zero phase-difference and the tone reported as of smallest volume. Both *T* and *M* were unable to detect difference of pitch in a series of over 25 paired presentations. Their results were supported by Dr. Pratt, who has a small limen for pitch discrimination. *B* tended to report differences of pitch; but his capacity for pitch discrimination is known on the basis of tests to be low. With this pair of tones he reported the smaller as higher 9 times, the larger as higher 11 times, and the two as equal 9 times. There is therefore no evidence of a discriminable difference in pitch between the two tones which

give the maximal volumic difference. Similar negative results were obtained from comparing the smallest tone with the lateral tone, and also from comparing the median tone and the lateral tone.

There remains the matter of the relation of volume to intensity. There is no doubt that the diotic tones varied intensively, and casual observation makes it appear as if the intensive function might be similar in form to the volumic. All *Os* mentioned the presence of intensive differences in their protocols. Occasionally they thought that their judgments might have been influenced by intensive differences, but at other times they were positive that they had not been so influenced. For them to have been on their guard against intensity as the basis of judgment gives more assurance that it was not the basis than if they had not mentioned it. On the other hand, it is doubtful whether under these conditions intensity can be separated from volume as was pitch. It is not impossible that the two are essentially covariant. The Bernstein hypothesis, which was responsible for the undertaking of this problem, would imply such a relationship, and, although these experiments have not brought out the function that a modified Bernstein theory implied, they do not disprove the relationship between extensity and intensity that is the basis of that theory. It may be that experiments would show that volume varies with the intensity of a tone in diotic hearing without phase-difference.

Conclusion

Diotically perceived tones vary in their volumic aspect when the phase relationship between them is altered. Medianly localized tones, where the phase-difference is zero, and laterally localized tones, where the phase-difference is half the wave-length, are largest. The intermediate phase-differences give smaller tones, and the minimal volume occurs for a difference in phase of about 3/16 wave-length. These volumic changes are of the same kind as the changes of volume that accompany changes of pitch. They are unaccompanied by changes of pitch under these conditions, but are accompanied by changes of intensity.

AN EXPERIMENTAL STUDY OF HENNING'S SYSTEM OF OLFACTORY QUALITIES[1]

By MALCOLM K. MACDONALD, University of Pennsylvania

The purpose of this study was the verification of the qualitative olfactory continuum represented by Henning's[2] smell prism. We hoped either to verify the general nature of the continuum or to discover necessary modifications of it. As secondary matters we hoped that we might be able to specify a practical set of stimuli for the demonstration of the prism and, by using stimuli of known chemical nature where possible, to obtain some incidental information bearing upon Henning's theory of the chemistry of the olfactory stimulus.

Henning represents the odors as lying in a qualitative continuum represented by the surface of a triangular prism.[3] At the six corners of this prism are the six principal groups of odors, which correspond to the *Hauptfarben* of the color pyramid as fundamental points of reference. At the corners of one triangular face of the prism lie the fragrant (F), ethereal (E), and putrid (P) odors, and at the other end respectively opposite these three are the spicy (S), resinous (R), and burned (B) odors. Along the edges and in the five faces lie psychologically simple and qualitatively intermediate odors, but the interior of the prism is hollow.

It will be convenient if we designate the principal odors at the corners as *simplex* odors. Such odors are the points of reference for the rest of the figure; they are fundamental and cannot be described qualitatively, whereas the other odors can be described by reference to them. Odors lying along an edge may be called *duplex* for the reason that they can be described by their qualitative similarity to two simplex odors. They are of course simple and not complex, but are defined in the qualitative system in a duplex manner. They may be designated as FE, EP, PF, SR, RB, BS, FS, ER, or PB. Within the faces there would be *multiplex* odors. At the middle of the rectangular sides (Henning's *Quadrupelpunkte*[4]) the multiplex odors are quadruplex: FESR, EPRB, and FPSB. In strict logic there should be no triplex odors in the rectangular sides, since any odor here should have fourfold similarity. Henning, however, appears at times to consider that odors near one corner and remote from the diagonal are practically triplex: dehydrocamphylcarbinol appears to be FSR[5].

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[1]From the Psychological Laboratory of Clark University. Communicated by Edwin G. Boring.

[2]H. Henning, *Der Geruch*, Leipzig, 1916, pp. 533. Reprinted from the *Z. f. Psychol.*, 1915, 73, 161-257(=pp. 1-97 of *Der Geruch*); 1916, 74, 305-443 (=pp. 98-226); 1916, 75, 177-230 (=pp. 227-280); 1916, 76, 1-27 (=pp. 281-407). *Cf.* E. A. McC. Gamble, *Psychol. Bull.*, 1916, 13, 135 ff.; this JOURNAL, 1921, 32, 290 ff.

[3]Henning, 80-97.

[4]Henning, 96, 299.

[5]Henning, 299.

The triangular ends should of course contain triplex odors, but Henning does not discuss these faces. This terminology is not Henning's, but is useful in representing his system.

Henning has a chemical theory[6] of the olfactory stimulus, which, if it held rigorously, would be a valuable aid in determining the stimuli for different regions of the prism. Odor, according to this theory, depends primarily upon the structure of the molecule of the stimulus, although it is also to some extent dependent upon the nature of the component elements.

The stimuli to the *simplex odors* have a single structural characteristic. The stimuli for the FESR face are all theoretically derivatives of the benzine ring. The PB line is characterized by the introduction of certain other elements as well as by molecular structure. The simplex stimuli are as follows.

Fragrant. An *ortho*-substitution product of the benzine ring, *i. e.*, substitution of chemical groups for the hydrogen at two adjacent corners of the six corners of the ring.

Spicy. A *para*-substitution product of the benzine ring, *i. e.*, substitution of chemical groups for hydrogen at two opposite corners of the ring.

Ortho-substitution and para-substitution are both forms of disubstitution. The only other disubstitution is meta-substitution, where the substitution is at the corners next but one. The meta-substitutions do not enter into the theory and should presumably be odorless. Monosubstitutions similarly do not appear in the theory. In a disubstitution product it makes no difference at which pair of corners the disubstitution occurs, provided only the spatial relation is fixed: all para-substitutions are identical for the same substituted groups, but different from orthosubstitution for the same substituted groups.

Resinous. An *inner* substitution in the benzine ring, *i. e.*, a disubstitution of a single group linking across between two corners of the ring and usually represented as within the ring.

Ethereal. A *hydrocarbon forked chain*, not necessarily related to the benzine ring, but discussed by Henning as if it belonged with the benzine derivatives. The theory is complicated at this point by the fact that there are few, if any, pure ethereal stimuli; nearly all are duplex ES and combine in the stimulus the fork with the para-substitution in the ring.

Putrid. *Forked chains* in which *sulphur*, *selenium*, *tellurium*, *nitrogen*, *phosphorus*, *arsenic*, *antimony*, *bismuth* are osmophores (smell producers; analogous to chromophores). Henning describes these forks as a disruption of the ring, but there seems to be no obvious chemical ground for this interpretation of a ring.

Burned. *Heterocyclic rings* including *nitrogen* with various numbers of sides. The benzine ring is homocyclic, since carbon is always the link, and it always has six sides. In a B stimulus nitrogen is substituted for carbon at one corner (not for hydrogen, as in the benzine derivatives).

The *duplex odors* have, on this theory, duplex stimuli, *i. e.*, stimuli that combine two of the simplex chemical characteristics. Thus in the FESR face they tend to be trisubstitution products. The stimulus to an FS odor combines the ortho- and para-substitution of the F and S groups. Such a ring is known as an asymmetrical ring. The stimulus to an SR odor combines inner and para-substitution; the FE stimulus should include a fork in one of the ortho-substitutions; and the ER stimulus should have a fork added to the inner substitution. There ought, on the theory, to be no FR odors which are not also E and S, and no ES odors which are not also F and R; and the corresponding duplex stimuli ought not to

[6]Henning, 281-305, esp. 291-301.

occur. Unfortunately for the theory, ES odors, as Henning admits, are the rule rather than the exception, and they are given by stimuli that add a fork to a para-substitution.

The duplex stimuli for the two faces which include the P and B corners are not well worked out by Henning, and the general principle of the theory cannot be extended here.

The faces should contain *quadruplex* stimuli, and possibly triplex stimuli, if there are any strictly triplex odors. The FESR face does contain both triplex and quadruplex stimuli. Thujone, the stimulus to the quadruplex odor at the center, illustrates the theory, for it is an asymmetrical ring (ortho- and para-substitution) with an additional inner substitution and with the para- connection carrying a fork.

The foregoing terms for the six principal olfactory groups have the advantage that they have different initials and provide a simple terminology for both simplex and multiplex odors (as do R, Y, G, O, RO, YG, *etc.*, for the colors). The usual English words and Henning's German which they translate are as follows.

F = Fragrant (flowery, *blumig*, *duftend*);
E = Ethereal (fruity, *früchtig*);
P = Putrid (foul, *faulig*);
S = Spicy (aromatic, *würzig*, *gewürzhaft*);
R = Resinous (piny, balsamy, *harzig*, *balsamisch*);
B = Burned (burning, scorched, empyreumatic, *brenzlich*).

Stimuli[7]

We sought to select stimuli which should represent all parts of the prism and which have been definitely placed by Henning. In addition we sought, when possible, to choose stimuli of definite chemical composition in order that we might obtain some incidental information concerning Henning's theory of the stimulus, and also for the reason that such a stimulus is more definitely defined in nature than is a common object. The selection proved peculiarly difficult, because Henning skimps many parts of the prism in his lists of representative odors. In some regions, where Henning cites definite chemical examples, we were unable to obtain the stimuli. For example, we did not succeed in finding thujone (FESR), camphene (ER), cadaverine (PB), chavicol (S), cineol (R), or cajuputol (R), in this country or in England. The PB edge does not permit of laboratory investigation; Henning had to take his *Os* to the zoological gardens to obtain these odors.

Table I gives a list of the stimuli which were used at any time during the experiment. A more definite characterization of these stimuli, and a statement of the reason for the rejection of many, follow. After the name of each substance is given its position on the prism according to Henning and in parentheses the page-reference to the statement in *Der Geruch*. It will be observed that, of these 35 stimuli, 20 are chemicals and 15 objects.

[7]Our thanks are due to Dr. George F. White, Professor of Organic Chemistry at Clark University, for his assistance in the selection and procuring of our stimuli and in obtaining information concerning their chemical structure.

TABLE I

LIST OF STIMULI. Standards are given in italics. An asterisk indicates a stimulus of definite chemical structure.

1	F	*Ionone
2	E	*Acetic ether
3	FS	*Vanillin
4	FESR	*Menthol
5	PB	*Amyl alcohol
6	RB	Burned mastic
7	B	Tar
8	B	**Pyridine*
9	B	*Nicotine
10	P	*Carbon disulphide
11	ER	*Xylene
12	SB	Burned coffee
13	P	*Asparagine
14	S	*Anisol
15	P	**Thiophenol*
16	R	Frankincense
17	FPSB	Celery
18	S	Nutmeg
19	R	Balsam of Peru
20	FESR	Wormwood
21	EPRB	Grapefruit rind
22	EP	Rotten fruit
23	FPSB	Onion
24	F	*Oil of jasmine*
25	FS	Vanilla
26	FE	*Citronellol
27	R	**Eucalyptol*
28	FPSB	*Apiol
29	S	**Anethol*
30	FE	*Geraniol
31	E	**Citral*
32	FP	Rotten flowers
33	SR	Allspice
34	SR	*Myrtenol
35	R	*Pinene

SIMPLEX STIMULI		
F	1	*Ionone
	24	*Oil of jasmine*
E	2	*Acetic ether
	31	**Citral*
P	10	*Carbon disulphide
	13	*Asparagine
	15	**Thiophenol*
S	14	*Anisol
	18	Nutmeg
	29	**Anethol*
R	16	Frankincense
	19	Balsam of Peru
	27	**Eucalyptol*
	35	*Pinene
B	7	Tar
	8	**Pyridine*
	9	*Nicotine
DUPLEX STIMULI		
FE	26	*Citronellol
	30	*Geraniol
EP	22	Rotten fruit
FP	32	Rotten flowers
SR	33	Allspice
	34	*Myrtenol
RB	6	Burned mastic
SB	12	Burned coffee
FS	3	*Vanillin
	25	Vanilla
ER	11	*Xylene
PB	5	*Amyl alcohol
MULTIPLEX STIMULI		
FESR	4	*Menthol
	20	Wormwood
EPRB	21	Grapefruit rind
FPSB	17	Celery
	23	Onion
	28	*Apiol

Simplex Stimuli

1. Ionone. Not mentioned by Henning. Synthetic violet; might be expected to belong to the F corner. It is a derivative of tetrahydro-benzine and a vicinal ring, which is equivalent to two ortho-substitutions, and thus may be said to accord with Henning's theory. Considered as a possible standard for the F corner, but rejected in favor of oil of jasmine which has a stronger odor. Used throughout the experiment.

24. Oil of jasmine. F. (H. 82) Used throughout the experiment as a standard for the F corner.

2. Acetic ether. E. (H. 83) Henning places all the ethers at this corner of the prism. Acetic ether involves the forked structure required by Henning's theory. Used throughout the experiment.

31. Citral. E. (H. 295) Used as a standard for the E corner throughout the experiment. It is an open chain with a fork, and readily condenses into a ring with para-substitution and a fork. Thus it approximately fits Henning's theory. Its structure would seem to imply that it should be ES (or FESR?) and not pure E. (*Cf.* no. 26.)

10. Carbon disulphide. P. (H. 301) Considered as a possible standard for the P corner, but rejected because its odor is said to be due to impurities. As an open chain involving sulphur it should, on Henning's theory, lie at the P corner. Used throughout Part I.

13. Asparagine. P. (H. 301) Occurs in the form of crystals as an open chain involving nitrogen. It had no perceptible odor for our *Os*, in spite of the fact that Henning gives it as typical of the P quality. The crystal was loaned us and we were not permitted to try dissolving it. Used only in the very first part of the experiment.

15. Thiophenol (*Merkaptane*). P. (H. 301) The aromatic mercaptan used was thiophenol. As a mono-substitution of a sulphur chain in benzine it is a proper P on Henning's theory. As obtained from the chemical laboratory it was so strong as quickly to permeate the whole room when the bottle was opened. The method hit upon was to hold an empty bottle over the mouth of the bottle containing thiophenol. In a few minutes enough of the stimulus had adhered to the inside of the bottle to last for some time, but the process had to be repeated often. Used throughout Part I as a standard for the P corner.

14. Anisol. S. (H. 292) A mono-substitution product of benzine; its para-derivatives Henning characterizes as S. In the shortened series it was eliminated in favor of nutmeg, which has a definitely spicy quality.

18. Nutmeg (*Muskat*). S. (H. 81) Whole nutmegs were grated as needed. Used throughout the experiment.

29. Anethol. S. (H. 292) Satisfactory S stimulus, and used as a standard for the S corner throughout the experiment. It is a para-substitution product as required by Henning's theory.

16. Frankincense (*Olibanum*, *Weihrauch*). R. (H. 84) Weak stimulus but strengthened by pulverizing. Used throughout the experiment.

19. Balsam of Peru. R. (H. 84) Eliminated because too weak a stimulus.

27. Eucalyptol. R. (H. 297, *cf.* 84) Satisfactory representative of the R corner and used as a standard for that corner. An inner ring as required by Henning's theory.

35. Pinene. R. (H. 297) A good chemical stimulus for the R corner. It accords with Henning's chemical theory, since it is an inner ring. Used throughout the experiment.

7. Tar. B. (H. 85) Coal tar was found to have only a faint odor, which was, however, strong enough when the tar was pulverized. Used throughout Part I.

8. Pyridine. B. (H. 85, 300) As pure pyridine was too strong, it was diluted with distilled water. Pyridine is a 'smooth' heterocyclic nitrogen

ring, and thus represents Henning's typical B both qualitatively and chemically. Hence it was used as a standard for the B corner throughout Part I.

9. Nicotine. B. (H. 300) Similar to pyridine both chemically and in Henning's qualitative classification. Hence should be satisfactory, but proved too weak to give good results. Used throughout Part I.

Duplex Stimuli

26. Citronellol. FE. (H. 296) Eliminated in favor of geraniol (no. 30) in order to reduce the number of stimuli on the FE line. It is an open chain with a fork and readily condenses into a ring with para-substitution and a fork, which should presumably under Henning's theory make it ES (or FESR?) instead of FE.

30. Geraniol. FE. (H. 296) Satisfactory FE stimulus, and used throughout the experiment. It is an open chain with a fork and readily condenses into a ring with para-substitution and a fork. It is therefore like citronellol and raises, because of its chemistry, the same theoretical objection. (*Cf.* no. 26.)

22. Rotten fruit. EP. Not mentioned by Henning, although seemingly implied as an intermediate. Early eliminated, however, because even when very rotten it was mostly like E. Rotten apples and oranges were used. It was difficult to keep this stimulus constant.

32. Carrion flowers (*Aaspflanze*). FP. (H. 85) The carrion flower was not available, but we used decayed flowers. They were, however, open to the same objection as decayed fruit (*cf.* no. 22) and were therefore eliminated.

33. Allspice. SR. (*Cf.* H. 34) Ground allspice. Used throughout the experiment as presumably representative of the SR line.

34. Myrtenol. SR. (H. 298) According to Henning a good representative of the SR line since it combines the inner and para-substitutions in the benzine ring; other texts, however, cite it as an oil of uncertain composition. Used throughout the experiment.

6. Burned mastic (*brennendes Mastixharz*). RB. (H. 85) The mastic gum on being burned forms a solid mass which has no perceptible odor. Discarded as an impractical stimulus. At best its odor is very weak.

12. Burned coffee (*gebrannter Kaffee*). BS. (H. 85) This was an unsatisfactory stimulus, for coffee exposed soon loses its odor and burning or roasting does not renew it. Eliminated during the first series.

3. Vanillin. FS. (H. 83) An asymmetrical ring and thus in accordance with Henning's theory. Used throughout the experiment.

25. Vanilla. FS. (H. 83) Vanilla beans. In the later series vanilla was eliminated in favor of the chemical vanillin (no. 3) which has a definite composition.

11. Xylene. ER. (H. 85) A disubstitution of CH_3 for H in a benzine ring. Does not seem to fit the chemical theory. Used throughout the experiment.

5. Amyl alcohol. PB. (H. 85) An open chain alcohol (a hydrogen, carbon and oxygen compound) with a fork, and should therefore on Henning's theory lie at the E corner and not along this edge. Used throughout Part I.

Multiplex Stimuli

4. Menthol. FESR. (H. 85) Henning places it between E and S, and it would seem, therefore, from the logic of the prism, to be also between F and R, although it is not given as a quadruplex stimulus (H. 96). Chemically, on Henning's theory, it should be a triplex (FES) stimulus, since it combines the para-(S), ortho-(F), and forked (E) structures. Used throughout the experiment.

20. Wormwood (*Wermutarten*). FESR. (H. 96) Unfortunately had to be eliminated because a very weak stimulus, although it is, according to Henning, the typical quadruplex stimulus of this face of the prism. Menthol (*v.* no. 4) is theoretically a representative of the interior of this face.

21. Grapefruit rind (*Pompelmusfrucht*). EPRB. (H. 96) Henning mentions grapefruit for the middle of the EPRB face. New rind had to be obtained frequently, as it decayed rapidly. Used throughout Part I.

17. Celery. FPSB. (H.85, 96) Celery seed. Celery according to Henning represents the quadruplex stimulus for this face of the prism. Used throughout Part I.

23. Onion. FPSB. (H. 85) Eliminated because celery (no. 17) represents the same place on the prism and is a more constant stimulus.

28. Apiol. FPSB. (H. 96) Satisfactory as a stimulus, and used throughout Part I. It is a complex penta-substitution product, apparently not related to Henning's chemistry of the prism.

Procedure. Henning insists on dirhinic smelling.[8] To ensure the use of both nostrils, 50 cc. glass-stopper, salt-mouth bottles (6.34 cm. deep, 2.54 cm. mouth-diameter) were used for presenting the stimuli. Henning also states that memory-smells, akin to memory-colors, fuse with the quality when the *Os* know the nature of the substances.[9] To pay regard to this admonition, the bottles were covered with paper and the *Os* were instructed not to look within. The different stimuli were identified by inconspicuous numbers on the covering of the bottles. The numbers had no relation to the position of the odors on the prism.

Standards. As a standard for the F-corner there was a choice among several of the essential perfume oils, oil of jasmine, oil of geranium, oil of ylang-ylang, and tonka bean, oppoponax, mimosa and coumarin. We selected *oil of jasmine*, which was of sufficient strength for a satisfactory standard, after rejecting the chemical ionone (*v. supra*).

For the E-corner there was a choice of the several ethereal oils, such as oil of lemon peel, oil of lime, oil of orange, as well as some of the ethers, *e. g.*, acetic ether. *Citral* was chosen as a standard on account of its known chemical nature as against the oils, and because it has a stronger and more distinct odor than the ethers.

For the P-corner there was the choice of carbon disulphide, hydrogen sulphide, the mercaptans, the cacodyls, and the stabines. Carbon disulphide was not used because its odor is said to be due merely to the presence of impurities. *Thiophenol*, one of the mercaptans, was selected because of its distinct strength of odor.

For the S-corner, from the ordinary spices and several chemicals, *anethol* was chosen on account of known chemical structure and availability.

Eucalyptol was chosen for the R-corner, from among frankincense, oil of cedarwood, tincture of myrrh, *etc.*, because of its known chemical structure.

From a number of chemicals at the B-corner, *pyridine* was chosen because sufficiently strong and easily available.

The standards thus selected were covered with paper, as were the other stimuli, but were prominently marked with large black gummed letters from I to N. These letters were chosen as having fewer associations than those at the beginning or end of the alphabet, and as having no relation to the names of the Henning classes or of the substances. The translation from the arbitrary symbol to the Henning class-symbol is as follows: I = F, J = E, K = P, L = S, M = R, and N = B.

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[8] Henning, 11-17.

[9] Henning, 26-39.

Observers

The *Os* who took part in this experiment were *H*, Dr. H. M. Halverson; *Ba*, Dr. Marjory Bates; and *Mo*, Miss E. F. Möller, all generally trained in introspection upon qualitative processes. They all had complete dirhinic olfaction. Another *O* was rejected because of monorhinic olfaction, due presumably to partial obstruction of one of the nasal passages. His inability to make satisfactory judgments in the preliminary experiments supports Henning's statement that dirhinic smelling is necessary for the complete appreciation of olfactory quality.

I. Test of the Prism by a Method of Paired Comparisons

The first method of experimentation was a form of paired comparisons, modified to suit the implications of the Henning prism. Every stimulus was presented to the *O* with every different pair of the six standards. He was allowed to smell the three stimuli in any order and as much and as often as he desired, and was instructed to report which of the two standards the comparison stimulus resembled more closely in olfactory quality. Half-hour periods of experimentation were used to avoid excessive adaptation. The frequency of the selection of the standards was computed for each stimulus; 'equal' judgments were counted one-half for each of two standards.

Table II shows the average and m.v. of nine series, three sets of observations with each of three *Os*. It is apparent from inspection of the table that variability is great and that no absolute verification of the prism was achieved. The original data show that this variability occurred both between *Os* and between the three series of a single *O*. In fact it was the large amount of variability that determined us to combine all series for all *Os* in the hope of finding some general tendency among observations which should support Henning's theory.

In examining the table it should be borne in mind that the maximal rank under a method of paired comparisons with six paired standards is 5: the ranks for a single stimulus, unless there are tie cases, should be 5, 4, 3, 2, 1, 0. The averages, however, show no such wide scatter of ranks; the scores are reduced and brought closer together by the large variability included in the average. Thus also the table shows that the m.v.'s are large with respect to the average ranks. If the results expressed in the table were perfectly self-consistent, the m. v. in every case would be 0; if they were inconsistent with maximum perversity the m. v. could be as high as 2.5. The average m. v. for the entire table (108 cases) actually is 1.12. This means, at any rate, that *Os* accustomed to introspective observation upon qualitative processes, and presumably indisposed to objectification of odors, could nevertheless not give judgments consistent with themselves or with one another when the task consisted in relating unknown stimuli qualitatively to the corners of the supposed prism. At least we may assert that the prism is by no means so easy to establish observationally as is the color pyramid.

TABLE II

Average ranks of the six principal qualities when their stimuli are compared by a method of paired comparisons with every one of the 18 comparison stimuli. The average is the average of 3 series for each of 3 *O*s, 9 values in all; it measures the relative degree of qualitative similarity to the quality named at the top of the column. The highest possible average rank would be 5, since there are 5 comparison pairs which include any one standard. The m. v. shows the variability; the maximum m. v. would be 2.5.

Comparison Stimulus		STANDARD STIMULI F		E		P		S		R		B	
No.	Class	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
1	F	2.5	2.1	2.4	1.6	.9	.9	2.5	.9	1.0	1.6	.7	.8
2	E	3.6	.7	3.7	1.0	1.0	.5	2.7	1.0	1.2	1.0	1.2	.8
10	P	0.0	0.0	0.0	0.0	3.5	1.2	1.1	1.4	.1	.2	3.5	1.5
18	S	2.8	1.3	3.5	.8	.8	1.0	3.2	1.5	0.0	0.0	.3	.6
16	R	.7	1.0	3.6	1.2	1.4	1.5	2.0	1.3	2.1	1.7	.9	1.0
35	R	1.3	1.5	2.0	1.8	1.5	1.1	1.3	1.4	3.1	1.6	1.5	1.3
7	B	.9	1.0	2.0	1.4	1.8	1.6	2.0	1.6	1.8	2.0	.9	.5
9	B	1.1	1.4	.8	1.0	2.3	1.6	1.7	1.8	.4	.6	1.9	1.7
30	FE	3.2	1.2	4.1	.4	.9	2.2	3.5	.8	.2	.3	1.1	1.0
33	SR	2.9	1.0	2.6	1.9	.9	1.2	3.4	1.0	.2	3.4	.4	.7
34	SR	.8	.9	2.0	1.4	1.3	1.2	2.3	1.9	3.1	1.6	2.0	1.5
3	FS	4.4	.5	2.8	1.2	.1	.1	3.6	1.2	0.0	0.0	.6	1.0
11	ER	.6	.6	1.6	1.5	3.7	1.1	1.8	1.5	2.2	1.2	2.4	1.5
5	PB	2.7	1.3	2.1	1.2	.7	1.0	2.7	1.4	3.7	1.4	.4	.6
4	FESR	3.1	.9	3.1	.7	.5	.8	3.2	.8	2.5	1.6	.4	.4
21	EPRB	3.7	.6	4.8	.2	.4	.6	2.5	1.1	.2	.3	.6	.8
17	FPSB	2.0	1.7	1.5	1.5	1.2	1.5	2.3	1.2	0.0	0.0	1.5	1.6
28	FPSB	.3	.5	1.2	1.4	3.2	1.6	2.4	1.8	1.0	1.2	1.5	1.3
Av. m.v.			1.01		1.12		1.14		1.31		1.09		1.03

It is interesting to note that the rank order of variability for the principal qualitative groups is, from the most to the least consistent, F, B, R, E, P, S. The low rank of E and S is undoubtedly due to the fact that the two groups, as Henning points out,[10] are related. Most fruits are also spicy in odor, and the forked structure is apt to be added to a para-ring. Hence actually the supposedly E stimuli are not pure E and may deviate in some unpredictable manner toward S. Similarly the S's under our method of experimentation may deviate toward E, for the reason that citral, the standard for E, approximates an ES stimulus. This defect lies in the Henning theory; we could not with assurance choose a better standard for E. Consistency is high for F and R, for these groups are more definitely establishable. Introspection shows F and R to be quite different in quality, and they are never confused as are E and S. In general, the PB portion of the prism is unsatisfactory because too few stimuli can be found adequately to represent it, and the positions of P and B in the rank order given above are less significant than those for the other four groups.

The variabilty, as shown in Table II, seems not to be due to lack of practice within the limits of our experiment, for the third series shows just as little agreement among *Os* as did the first series.

In the face of such great variability the natural resort is to statistical procedure. We have therefore correlated the theoretical ranks, which would obtain if the three sides of the Henning prism are squares, with the observed ranks.

It will be seen that the theoretical ranks are somewhat different for simplex, duplex and multiplex stimuli. For a simplex stimulus the quality for the corner at which it lies should rank 5; the three corners, equally distant from this corner by the side of a square, should each rank 3, and the two remaining corners, distant by the diagonal of the square, should rank 0.5 each. With a duplex stimulus the two adjacent corners should rank 4.5 each, and the four remaining corners 1.5 each. With a quadruplex stimulus the four equidistant corners should rank 3.5 each; and the two remaining corners should receive no score at all, for the reason that the prism is hollow and there is no single continuum leading to the opposite edge whereby a judgment of similarity can be made. Theoretically the ideal series of ranks would be:

For a simplex stimulus: 5, 3, 3, 3, 0.5, 0.5.
For a duplex stimulus: 4.5, 4.5, 1.5, 1.5, 1.5, 1.5.
For a quadruplex stimulus: 3.5, 3.5, 3.5, 3.5, 0, 0.

The coefficients of correlation (products-moments) between these theoretical and the observed values are as follows:

Standard Stimulus:	F	E	P	S	R	B
r=	.360	.432	.309	.334	.435	.119
P.E.r =	.138	.129	.144	.141	.129	.157

The average correlation is r=.331. The low value for B (r=.119) is explained by the fact that a potentially B stimulus tended to yield to all the other five groups in judged similarity; *cf.* Table II.

[10] Henning, 295.

In general, then, we may say that we succeeded in verifying the Henning prism only to the extent of agreement that is indicated by a coefficient of correlation of 30% to 40%. We shall see later some of the difficulties which accounted for this rather unexpected failure.

II. Test of the FESR Face by a Method of Qualitative Placement

Since the PB line and its adjacent faces could be only incompletely filled in, we decided to confine our further experimentation to the FESR face, for which numerous representatives were available.

The *Os* were trained to make judgments of qualitative relationship in the following manner. A card 15 inches on a side was divided into 25 three-inch squares. Twenty-five small colored squares about one inch on a side, all between red and blue and of various tints, a systematic selection from the Milton-Bradley series, were cut from colored papers. A light red, a dark red, a light blue and a dark blue were placed at the four corners of the square, thus indicating a red-blue bidimensional qualitative continuum in which the hue varied in one dimension and the tint in the other. The *Os* were asked to place, separately, each one of the other 21 colors in the continuum in qualitative relationship to the four standards at the corners. They found no difficulty in performing this task accurately.

These conditions were now duplicated for the FESR face of Henning's prism. The F, E, S, and R standards were substituted for the colors at the corners of the large square, and the *Os* were asked to place singly on the square every one of the 11 olfactory stimuli of the FESR group in qualitative relation to the standards. A series consisted of the placement of each of the 11 stimuli. Ten such series were taken, each at a separate session. The results are shown in Table III and in Figg. 1-3. In the figures the circles show the position of average placement for such stimuli as the *O* was able to place as many as five times out of 10 trials, and the lines run from the positions of actual placement to the average.

All the *Os* experienced difficulty in making the placements, a difficulty which they did not have in the preliminary trial with the colors. They responded differently, however. *Mo* accepted the instruction and managed every time to find a place for every stimulus; *Ba* found a place for only three of the 11 stimuli five or more times. *Ba* was never well satisfied with her judgments and worked under protest. No stimulus did she place more than seven times out of 10; every stimulus she placed at least once. Altogether she made 36 placements out of a possible 110. *H* placed eight stimuli five or more times and made 76 placements out of 110.

Table III

Average placement of 11 stimuli by 3 *O*s (*Ba*, *H*, *Mo*) in the FESR face of the prism. x = horizontal displacement from the FS edge toward the ER edge; y = vertical displacement from the SR edge towards the FE edge. All figures are in units defined by length of side = 10 units, *i. e.*, the maximal horizontal or vertical displacement is 10 and the maximal diagonal displacement is 14.14. Average displacement is the average deviation of the placements from the average position shown in Figg. 1-3. Ten trials for each stimulus were given each *O*, but *H* and *Ba* did not always succeed in making a placement within the square. When the number of placements is less than 5, no averages have been computed.

Comparison Stimulus		No. Cases			Average x			Average y			Average Displacement		
No.	Class	*Ba*	*H*	*Mo*	*Ba*	*H*	*Mo*	*Ba*	*H*	*Mo*	*Ba*	*H*	*Mo*
1	F	6	10	10	2.50	8.92	3.25	6.66	9.91	7.25	2.83	1.27	2.23
2	E	1	10	10	—	7.25	5.50	—	8.25	5.50	—	2.71	2.13
3	FS	3	4	10	—	—	3.00	—	—	8.50	—	—	1.79
4	FESR	3	10	10	—	5.75	4.75	—	4.25	4.66	—	1.79	2.04
11	ER	1	1	10	—	—	7.00	—	—	4.25	—	—	2.21
16	R	7	0	10	3.58	—	4.75	4.25	—	8.00	3.07	—	1.50
18	S	2	9	10	—	0.58	3.50	—	3.00	4.25	—	1.52	2.17
30	FE	3	10	10	—	3.75	5.25	—	4.75	5.05	—	2.71	1.06
33	SR	2	5	10	—	0.50	4.25	—	4.00	4.50	—	1.54	2.25
34	SR	5	9	10	6.50	10.00	7.75	2.50	3.33	6.25	2.46	1.56	1.38
35	R	3	8	10	—	10.00	6.00	—	1.58	5.50	—	1.14	3.06
								Average			2.82	1.70	1.98

The figures and Table III show that the *O*s varied considerably. *H* was the most consistent *O*: on the average his displacement of a stimulus from its average position was 1.70, where the length of the side of the square is 10. *Mo* is the next most consistent, with an average displacement of 1.98. Her greater variability may be due to her acceptance of the instruction in such a way as to lead her to place all the stimuli. *Ba*, however, who refused to make placements because of her lack of assurance, is still less consistent for the three bottles which she placed 5, 6, and 7 times; her average displacement is 2.82.

We took protocols from all the *O*s concerning their procedure in making placements. *H* and *Mo* both reported that they began by selecting the one of the four standards that most resembled the comparison stimulus. Then they found the standard or standards which next most resembled the comparison stimulus, and placed it with respect to all the standards that had come under consideration, correcting the placement with respect to the remaining standard or standards later. *Mo*'s procedure was more fixed than *H*'s. After deciding upon the first standard, she always went on to place the comparison stimulus in relation to the two adjacent standards, ending with a final correction which took account of the standard diagonally opposite. *Ba* gave little account of method, since she was more concerned with the rejection of stimuli than with their placement.

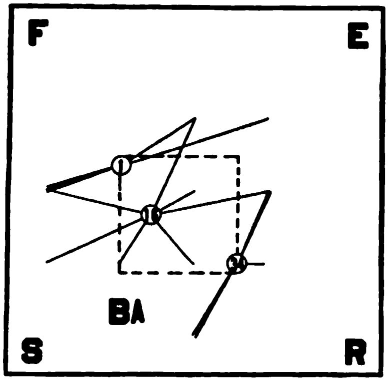

FIG. 1

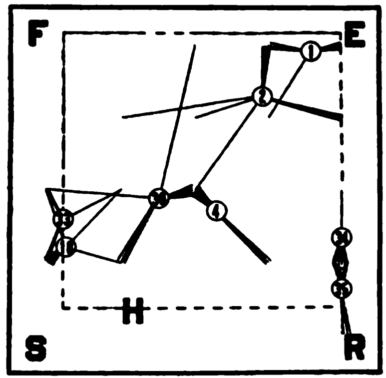

FIG. 2

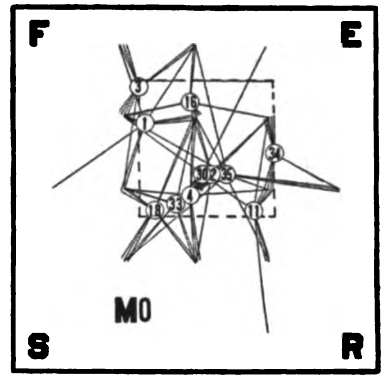

FIG. 3

FIGS. 1-3. Average position and displacements, indicated by radial lines, of the stimuli which *Ba*, *H*, and *Mo* placed in the FESR face of the prism five or more times out of ten trials. See Tables I and III. The dotted lines indicate the "reduced squares" used in computing the average deviation from theory. The key to the stimulus numbers and their classification is as follows:

1	F	Ionone	4	FESR	Menthol	18	S	Nutmeg	34	SR	Myrtenol
2	E	Acetic ether	11	ER	Xylene	30	FE	Geraniol	35	R	Pinene
3	FS	Vanillin	16	R	Frankincense	33	SR	Allspice			

All three *Os* had a great deal to say of the dfficulty of the task. Resemblance was not apparent, as with the colors, and the odors sometimes seemed complex.

After the completion of the main series with the FESR face of the prism, we conducted a supplementary series in which each *O* was asked to place every stimulus once and to give a full account of the method of placement.

These supplementary protocols accorded in general method of placement with the accounts given during the main series mentioned above. In addition, however, they brought out certain typical difficulties of the task which bear directly on the nature of the prism. These difficulties may be summarized as follows.

(1) Occasionally a stimulus is reported as resembling none of the four standards and is placed off the square. (No. 33, allspice, for *Ba*; no. 35, pinene, for *Mo*.)

(2) Frequently stimuli resemble a standard and some other unknown quality that does not lie in the square. Such qualities belong to some extension of the continuum beyond the square, and are properly placed off the square. (No. 3, vanillin, for *Ba* and *H*; no. 4, menthol, for *Ba*; no. 11, xylene, for *H*; no. 16, frankincense, for *H*; no. 30, geraniol, for *Ba*; no. 33, allspice, for *H* and *Mo*; no. 35, pinene, for *Ba* and *H*.)

(3) Occasionally a triplex quality is reported as about equally similar to three standards but not resembling the fourth. Strictly speaking, such a quality can not be placed in the square, since its placement with respect to three corners immediately implies a relationship to the fourth. As a matter of fact *Mo* placed such stimuli near the center of the square in spite of the incorrect implication. (No. 2, acetic ether, for *Mo*; no. 30, geraniol, for *Mo*.)

(4) Sometimes there is reported a duplex quality resembilng diagonally opposite standards. Such a quality also ought not to be placed, since its location near the midpoint of a diagonal puts it also at the midpoint of the other diagonal and renders it a quadruplex quality. In three of these cases the stimuli were actually, though thus incorrectly, placed near the middle of the square; in one case *Ba* refused to make a placement because of the incompatibility involved. (No. 1, ionone, reported ES by *Ba* and not placed; no. 4, menthol, reported FR by *H*, although Henning calls it ES; no. 13, nutmeg, reported ES by *Mo*; no. 30, geraniol, reported ES by *H*.) It is to be recalled that Henning especially notes the existence of the duplex ES, although he does not explain the inconsistency that it implies in the prism.

It will be noted in the figures that the placements tend toward the center of the square. Various factors may account for this result. (1) Since the placements were bounded by the edges of the square, chance placement would tend toward the middle. (2) The odors were noted for their dissimilarity from the standards. Even on Henning's theory, two F's, for example, may be very different sensations. Since the four standards only were given, the tendency must have been to place the stimuli well away from the corners. (3) It is possible that the method of placement which *Mo* employed led her to put most of her comparison stimuli near the center. By invariably bringing the stimuli into relation with three standards she tended to favor placements in the positions of multiplex stimuli and to discourage placements as duplex and simplex odors. In examining

the figures, then, we must take into account the tendency toward the center and attend more to the relative placement than to the actual position in the given FESR square.

We may now examine the results for each of the stimuli separately, noting the stimulus number, the substance, the Henning classification, and the *O*s who placed it five or more times.

1. Ionone. F. *Ba, H, Mo.* Chemically it should be a good F. *Ba* and *Mo* place it toward F in their constellations, but *H* consistently approximates it to E.

30. Geraniol. FE. *H, Mo.* Henning classifies it as FE. Chemically however (see above) it should be ES, which under the logic of the square continuum should become FESR. Both *O*s localize it in the center, as its chemistry on Henning's assumptions would require.

2. Acetic ether. E. *H, Mo.* Chemically also it should be E. *H* places it near E and *Mo* places it near the center; *H*'s greatest deviations are, however, toward the center and *Mo*'s toward E.

11. Xylene. ER. *Mo.* Supposedly ER, but does not accord with the chemical theory (see above). *Mo* places it consistently near the ER side of her constellation.

16. Frankincense. R. *Ba, Mo. Ba* makes it central and *Mo* places it toward the FE side. It does not seem to be R. It was, however, a weak stimulus and less satisfactory on this account than others.

35. Pinene. R. *H, Mo.* Also belongs chemically at R. *H* places it at R with great consistency, but *Mo* places it near the center.

33. Allspice. SR. *H, Mo.* We assumed that it should be SR from Henning's account of this line. It is probably to be considered an S since it is close to no. 18.

34. Myrtenol. SR. *Ba, H, Mo.* According to Henning this is also chemically SR, although we have not been able to verify his formula. *H*, however, makes it definitely ER; *Mo* locates it toward ER; and *Ba* puts it as near ER as SR.

18. Nutmeg. S. *H, Mo.* Both *O*s place it toward the S corner.

3. Vanillin. FS. *Mo.* She makes it F rather than FS.

4. Menthol. FESR. *H, Mo.* According to Henning it is a duplex ES; according to its chemistry it should be triplex FES; according to the logic of the square continuum it would have to be FESR if it were ES. Both *O*s conform to Henning's statement by placing it near the center. It does not seem to tend toward the F side, however.

It is difficult to make any general statement of the agreement of these results with Henning's theory. So many factors enter in, and agreement can occur or fail in so many ways. It is, however, possible to measure the deviation of the average position from the theoretical place for each stimulus, but in so doing we encounter the difficulty that arises from the tendency of all *O*s to group the placements in the center of the square. We have shown the possible causes for this tendency, and it does not seem fair to include it as a factor in any measure of the relationship between observation and theory. Accordingly we undertook to correct for it by redrawing the squares of Figg. 1-3 so that they would just include all the average points (see dotted squares in the figures) and computing the average deviation, for each *O*, of the average placement from the theoretical position. If the side of the reduced square be taken as 10 units,

then these average displacements in terms of such units are as follows: for *Ba*, 4.85; for *H*, 4.23; for *Mo*, 5.35. In other words, the discrepancy between theory and actual placement is on the average about one-half the length of a side of the square for every *O*. It does not appear, therefore, that agreement with theory is very much greater in these series with the FESR face than the correlation of 30-40% that we found before for the entire prism.

Logical and Factual Inadequacies of Henning's Theory

1. The prismatic theory of odor may be said in general still to be insufficiently worked out. For certain portions of the prism Henning gives numerous examples that should enable one more or less readily to reconstruct the qualitative system in experience, but in other portions a definite knowledge of the necessary stimuli is still lacking.

Thus a discussion of the internal constitution of the triangular bases of the prism is entirely lacking; presumably triplex odors should lie here.

Moreover, stimuli for the two faces which include the PB line are insufficient. Henning himself noted the difficulty of filling out the PB line, but the adjacent faces are also inadequately represented. We obtained no satisfactory stimuli for the FP, EP, SB, and RB lines, and Henning gives no definite chemical substances that belong to these positions. With respect to the quadruplex odors belonging to these faces, we find that FPSB can be adequately represented by the celery lactones, the onion esters, and the parsley phenols, of which apiol is an example; but the EPRB face has only the indefinite odor of grapefruit to represent it.

Another insufficiency appears at the E corner. Most of the fruity odors, which belong to this group, are actually similar to S, so that they are not proper representatives of E. Henning's own statement, the chemistry of these odors according to Henning's theory, and our own qualitative findings accord here.

2. Henning's chemical theory also presents insufficiencies in the FPSB and EPRB faces. It is not made clear how chemical intermediates between P and B, on the one hand, and F, E, S, and R, on the other, can occur or what the nature of an intermediate would be. The lack of examples makes it impossible to deduce generalizations. There is no apparent chemical reason why apiol, for example, a complex penta-substitution product, should be a quadruplex FPSB stimulus.

3. The question arises whether the FESR face is properly a square. The close relationship of the E and S corners suggests that the ES diagonal may be shorter than the FR diagonal. It may be, however, that the face is theoretically a square and

that the best practicable stimuli for E are ES, with pure E lying diagonally out beyond, very much as the corners of the theoretical color triangle lie beyond the actual spectral triangle enclosed within it.

4. The existence of triplex odors in a face seems to constitute a logical difficulty for the prismatic theory, in which the odors on a line should be duplex and the odors in a face quadruplex. Henning, however, seems to admit the existence of triplex odors (*e. g.*, thymol, p. 299, and dehydrocamphylcarbinol. p. 299), and our *Os* also report triplex odors which they have for this reason difficulty in placing upon the square (*e. g.*, acetic ether, FES, and geraniol, FER, for *Mo*).

5. An even more fundamental logical difficulty with the prism lies in the fact that there seem to be duplex odors for the diagonally opposite corners. Henning especially notes the usualness of the ES odors, and our observer *H* reported menthol as FR. If the prism is to stand, it is absolutely necessary that ES stimuli should also be FR and conversely, for only quadruplex odors could lie at the center of the diagonals. The trouble seems to be with the prism and not with Henning's observations; our *Os* frequently reported ES odors.

The solution may lie in some other geometrical construction If a solid tetrahedron were substituted for the FESR face, it would be possible to have all duplex, triplex and quadruplex odors that could lie between these four corners. If the other faces were similar, we might have a hollow hyper-solid with solid tetrahedrons as its sides. There is no reason why mental continua should occur only under Euclidian limitations.

6. This same difficulty with the ES odors appears also on the side of the stimulus. ES stimuli are normal, for the forked structure is found attached to the para-substitution. If Henning's theory were to be taken strictly there should be no such stimuli nor combinations of inner with ortho-rings (FR); the combination of one pair of these characteristics should necessitate the addition of the other pair. Geraniol, citral, and citronellol all violate the logic of the prism, in that they combine para-substitution with a fork without further additions.

7. Similarly, there should be no chemically triplex stimuli, since the position of the odor of such a stimulus in a face implies a similarity to the fourth corner. The case of menthol illustrates both this difficulty and the one preceding. Henning describes menthol as ES, a duplex odor, and *H* called it duplex FR. Chemically, it combines the ortho- and para-substitution with a forked structure, and should be a triplex stimulus, FES. Logically, however, since it lies in a face, it should be a quadruplex FESR.

8. A general difficulty with the Henning theory lies in the fact that it seeks to explain a qualitative continuum by correlation with discrete chemical changes. From S to F, for example, there is only one possible chemical intermediate, the asymmetrical ring that combines the para- and ortho-substitution, whereas there are apparently many qualities, with perhaps the possibility of continuous qualitative change.

9. In basing the FESR face on the benzine ring, it must be noted that Henning is interpreting the nature of the E stimuli rather broadly. These molecules, bearing a forked structure, are open chains and not rings. Their relation to the benzine group lies only in the fact that they may readily condense into rings.

The molecules of the P stimuli Henning describes as disrupted rings (*Aufsplitterung der Ringe*), although they are open chains without relation to the cyclic molecules. It would seem that the emphasis should be placed, as we have had occasion to do above, upon the nature of the osmophores and not upon the molecular structure.

10. The chemical theory does not permit rigorous prediction of quality, if Henning's qualitative descriptions are accepted. Among our own stimuli the following inconsistencies appeared. Citral, described as E by Henning, should be ES on the basis of its molecular structure. Citronellol and geraniol Henning seems to characterize as FE: their structure demands that they be ES: they include no ortho-substitution products that would make them F. Menthol, as we have seen, is called ES by Henning, when chemically it is FES. Amyl alcohol is an open chain with a fork and belongs chemically at E, but it is one of the few examples besides the animal foetors that Henning places on the PB line. Xylene is a disubstitution product and might be expected to be F or S; it is placed by Henning on the ER line. Carbon disulphide has a proper formula to represent the P corner, but its odor is said to be due to impurities.

11. Criticism of the chemical theory has necessarily been based on the internal consistency of Henning's own presentation. Criticism of the prismatic theory of olfactory quality is the main purpose of the present study. In it we have shown by a method of paired comparisons, involving stimuli that should represent the entire prism, that we could obtain only 30-40% correlation between the qualitative relationships as stated by Henning and as observed by our *O*s. We have also shown that, in placing stimuli within the FESR face, our *O*s, even when allowance is made for their tendency to place all unknowns toward the center of the square, deviated from Henning's placement by an amount equal to about one-half the side of the square. Both these results, however, issue from data

where individual variation is large, so that it may be argued that our failure better to verify the theory may be due to some fundamental difficulty in observing the qualitative relationships implied. The existence of such difficulties was apparent in certain series where the *Os* found some unknowns related to qualities not represented in the system and (rarely) to no qualities in the system at all.

It is the opinion of the writer that Henning's theories represent an advance, a first approximation to the truth. They can not, however, be applied rigorously, nor does the smell prism represent so definitely understood a system of qualities as does the color pyramid.

Note on Dimmick's Experiment

Since the acceptance of this manuscript for publication Dimmick has published "A Note on Henning's Smell Series."[10] Our proof sheets do not afford an adequate opportunity for the comparison of Dimmick's experiment with ours, but subsequent discussion may be avoided if certain divergences between the two experiments are noted here.

Both experiments are prejudiced to some extent by necessary methodical presuppositions. Dimmick assumed the validity of the prismatic theory of quality and sought to determine the best or most constant representatives of the theory; his study can not constitute a critique of the theory, but has the advantage of determining the best principal odors under the theory. Our study, on the other hand, raises the question of the validity and degree of applicability of the prismatic theory, but necessarily had to assume specific stimuli as representative of the principal classes of odor.

Our effort to obtain standards of definite chemical composition renders comparison difficult. Dimmick justifies our F standard (oil of jasmine), and raises a presumption in favor of the appropriateness of our E and R standards (if citrol and eucalyptol can be regarded as equivalent stimuli to lemon oil and eucalyptus oil). We are unable to come in contact with Dimmick's experiment at all with respect to the S and P corners. Pyridine, our standard for B, Dimmick places on the PB line.

There are only seven stimuli identical to the two experiments, and six more which are similar, as, *e.g.*, anisol and anise oil.

There seem to be a few instances (we have noted six) where Dimmick and we differ in the interpretation of Henning's specification of a stimulus. These discrepancies, however, are, with one exception, never greater than the difference between a corner and a line leading to that corner; and in these matters Henning is remarkably difficult of interpretation.

In general we note that the greatest difficulty in the test of the theory consists in the difficulty of establishing in advance the principal classes of odors to which the unknowns are to be referred in the qualitative schema. Given R, Y, G, B, Wh, and Bk, it is easy enough to demonstrate the validity of the color pyramid, but there seems to be as yet, in spite of these two experiments, no similarly easy mode of determining the principal olfactory points of reference. *Hauptrot*, for example, may be difficult of exact determination, but the general concept of "redness" can be easily be given to an *O*, much more readily than can the concept of "fragrance" or a *hauptblumig* odor. Moreover, it would seem that the various F's are very much more different from one another than is any group of colors all of which could be called "red."

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[10] F. L. Dimmick, this Journal, 1922, 33, 423-425.

A STUDY OF LIMINAL SOUND INTENSITIES AND THE APPLICATION OF WEBER'S LAW TO TONES OF DIFFERENT PITCH

By **Martha Guernsey**, University of Michigan

That the normal human ear is not uniformly sensitive to all gradations in the tonal scale has become a recognized physical fact. The absolute limit of that sensitivity, however, and a measure of the absolute intensity required to elicit response to different pitches are factors which, so far, have never been wholly determined. Very slow and very fast vibrations we either do not hear or else hear uncertainly; and the character of these turning points, *i.e.*, whether they are sudden disappearances or gradual declines, as well as the possibility of varied sensitivity to pitches within these boundaries, have proved an interesting problem both from a physical and a psychological point of view.

In reviewing the literature on auditory phenomena, we find that the bulk of sound experiments comprises such phases as pitch discrimination, the much mooted question of tonal attributes, and physiological theories, with a very meagre contribution in the nature of quantitative measurements of intensity or the audible limits. The material in the following table (A), taken with permission of Professor Pillsbury from the *Psych. Review Mon. Supp.*, 13, 1911, and supplemented with an addition from Gildemeister, summarizes practically the whole of the available quantitative data.

Many of the earlier psychological investigations of sound were carried on with freely falling balls and pendulums; but these apparatus tended rather toward controversy over physical formulae than toward adequate conclusions. For his measurements of sound intensity, Lord Rayleigh used metal cans with vibrations induced through an electromagnet. His results are larger than Wien's, and are limited to determinations of just two pitches in the lower middle range. Toeppler and Boltzmann utilized closed tubes, in which the concentration of sound was measured by a method of interference.

Tuning forks, both electrically and simply driven, have furnished a considerable bulk of experimental data. They were employed in the investigations of Zwaardemaker and Quix, Wead, Stumpf, and by some of the more recent experimenters, all of whose results nevertheless refuse to harmonize on the ground of a common apparatus. This disparity may be due to

TABLE A

N	$WN.$	$R.$	$ZW.\&Q$	$WD.$	$T.\&B.$	$G.$
32						Upper limit (15000–20000 vd.)
50	4.10^{-6}					0.2–0.5 watt
64						
96			$2,8.10^{-1}$			
100	7.10^{-9}					
128			$2,7.10^{-1}$			
181					3.10^{-6}	
192			$4,6.10^{-1}$			
200	3.10^{-11}					
256		$8,5.10^{-9}$	$5,5.10^{-2}$	83.10^{-6}		
384		6.10^{-9}	$3,4.10^{-2}$	28.10^{-7}		
400	3.10_{-14}					
512			$1,97.10^{-3}$			
768			$2,5.10^{-4}$	31.10^{-7}		
800	7.10^{-15}					
1024			$2,7.10^{-4}$	11.10^{-7}		
1536				22.10^{-6}		
1600	1.10^{-16}					
2048				71.10^{-7}		
3200	5.10^{-16}					
6400	3.10^{-15}					
12800	5.10^{-14}					

The values in the first column represent the rate of vibration of the various tones. The succeeding columns include the energy in ergs required for minimal intensities of these tones as determined respectively by Wien, Rayleigh, Zwaardemaker and Quix, Wead,[1] Toeppler and Boltzmann, and Gildemeister.

[1]Wead's measurements represent the energy in the tuning fork itself. The energy affecting the ear would be much less.

the difficulties inherent in the tuning forks themselves, or perhaps to differences in the psychophysical methods of acquiring results.

A recent experiment dealing primarily with the upper audible limit, but affording also its quantitative equivalent in energy, is that of Gildemeister. His apparatus utilises a condenser and induction coils, and has many factors in common with the apparatus we employ in our work at Michigan.

Weiss has also investigated the sound-intensity reaction, using electrically driven forks and shifting resonators. His aim is not so much to determine actual limens as to secure evidence for his physiological theory; but some of his results correlate with pertinent phases of Weber's law. He found that when the resonators are near the fork the just-noticeable differences are shorter steps than when the resonators are farther away, and that this increase in length is approximately a logarithmic series. Weiss found also a rather marked disparity between individual discriminations within the same "critical range."

In reviewing apparatus, a mention of the transmitter of Wente and the thermophone of Arnold and Crandall should not be omitted, although pertinent results from their work are not yet available.

It is Wien's determinations of liminal sensitivity, however, which are of the most relevant interest here. His work, in fact, affords probably the most authoritative, and certainly the most comprehensive reference for tones of weak audibility; and his apparatus, based on the theory that the energy of the increasing stimulus serves directly as a measure of the mass of the sensation, has provided a workable model for use in our own telephone set-up. He has left results, however, only for tones ranging from 50 vd. to 13,000 vd., and the validity of these is somewhat lessened by the fact that he apparently used only himself as subject, thus omitting the factor of individual differences in auditory acuity.

As the manuscript is finished the articles of Bunch and Zuehl appear in the University of Iowa studies. They used a method of inducing the pitch by varying the rate of rotation of an armature near electromagnets. The results are given only relatively in terms of the resistance required to reduce to zero the tone produced by a standard current. This procedure neglects the fact that the intensity of the sound varies as the square of the rate of vibration as well as with the amplitude. The rapid falling-off in sensitivity above 750 vd., which Zuehl's curves show as compared with our measurements and Wien's, is to be explained by this fact.

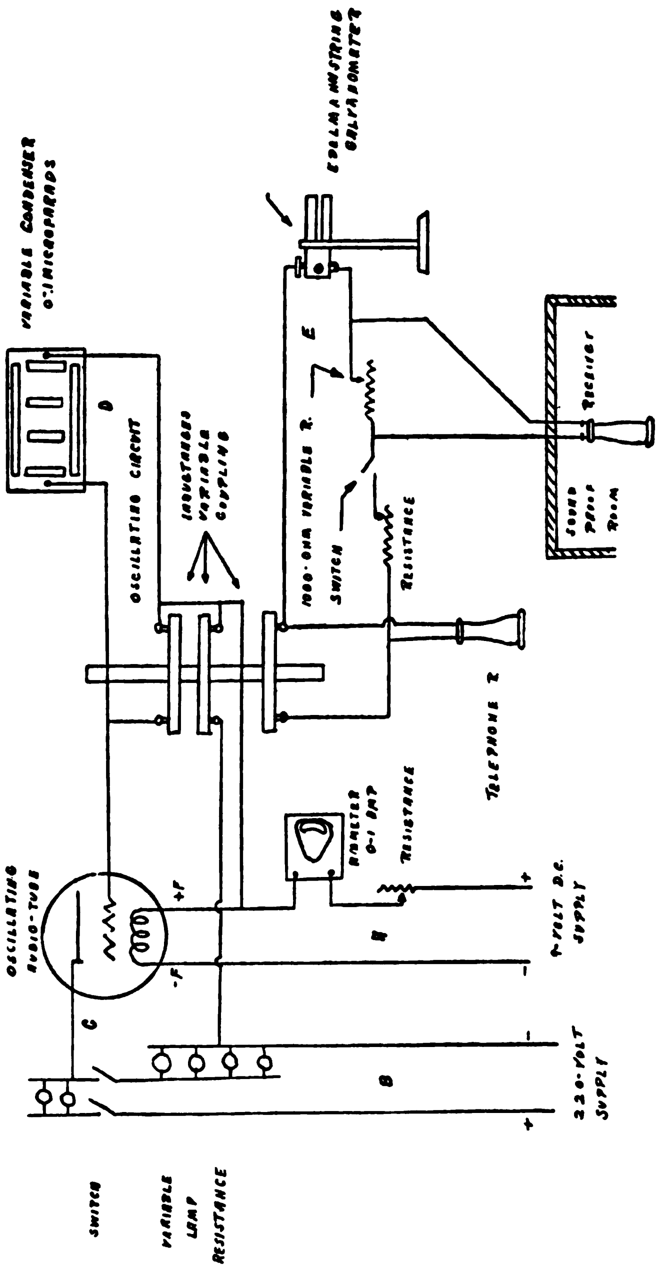
The Helmholtz resonance theory favors a hypothesis of sudden turning points of intensity in the tonal scale. Wien has challenged this assumption by a series of experiments which, though maintaining definite turning points, makes them rather gradual, and recurring through several octaves. In general, Wien found that the curve of liminal intensity in hearing increases markedly from the lowest audible tones to those of 3,000 vd., where it begins to diminish, at first slowly, then more rapidly. The region between 1000 and 5000 vd. elicits the greatest sensitivity. These latter results are apparently substantiated by the work of Gildemeister, though only one definite measurement is offered in his article.

From the foregoing brief review, it will be seen that the apparatus hitherto used for determining sound intensity roughly classifies into five main divisions: (1) use of pendulums, falling balls, etc.; (2) use of discs and resonators; (3) use of tuning forks and electromagnets; (4) use of direct optical methods for observing the motions of a vibrating diaphragm; and (5) use of telephone transmitters with subsidiary apparatus. Our apparatus in the laboratory at Michigan uses a combination of the last two.

All of the methods, of course, presuppose the same physical and physiological factors of sound phenomena: namely, that the intensity of the sensation depends in rough measure upon the kinetic energy of the vibrating medium, and that at the same time the ear, in reporting it, is affected by subjective conditions of perception. The same intensity, for instance, will appear to be different for different *O*s, and even for the same *O*s on different days or under different experimental conditions, and this factor has added decidedly to the complexity of establishing rigid measures of absolute intensity. Our experiment has consequently been conducted on a quantitative basis, and has consisted of taking a large number of measurements of minimal intensities at different vibration rates, using trained *O*s under controlled conditions.

Apparatus

Our apparatus utilizes vacuum-tube oscillation, thus providing a novel method of obtaining pitch differences by electrical tuning such as that used for high frequency amplification in wireless communication. The principal factors in the tuning circuit are a vacuum tube, coils for self-induction, and a condenser of variable capacity for changing the pitch. To the induction circuit is also attached a galvanometer which provides a direct means of measuring the amplitude of oscillation of the current. Two current sources are employed, one from a battery of sufficient voltage (9–12 volts) to heat the filament of the tube,



APPARATUS PRODUCING LIMINAL INTENSITIES.

the other from a power source of 220 volts d.c. The grid is in series with one coil and the condenser; the plate, with a second coil. We thus have two currents which, through mutual induction, start each other into oscillations at a rate determined by the amount of self-induction and capacity in the circuit.

The telephone circuit is composed of a third coil, the telephone receiver, a known variable resistance, and an Edelmann string galvanometer. The last instrument is substituted for the dynamometer of Wien, with the probable advantage of increased delicacy and accuracy. Calibrated to linear measurements, a single scale division is equivalent to 1/242 mm. vibration of the plate.

The different pitches are established by varying the capacity, and these resulting tones are measured by comparing them with Edelmann forks and Galton whistles. For higher tones, ranging from 4,000 to 13,650 vd., we use smaller inductance coils with higher frequencies, and the frequencies are computed from capacity and inductance. In the table of results those tones which were determined directly are indicated by *d*. Those which were computed are preceded by *c*. The method of computation for the higher tones is relatively simple, employing the known measurements of capacity and inductance in the formula $f = \frac{1}{2\pi\sqrt{LC}}$. We could determine L from the known capacity and pitch for several pitches; and we then used that with readings from the condenser to determine the other unknown pitches. Probably what we called L was partly C in parts of the circuit other than the condenser, but that would not affect our comparisons.

For a more adequate idea of the apparatus itself, the accompanying diagram may help.

A supplies filament with current to heat it, thus providing an electronic discharge from the filament to the plate.

B provides an independent voltage from the plate to the filament; controllable by lamps.

C Grid circuit through middle inductance.

D Oscillating circuit composed of variable condenser and upper inductance, producing by its oscillations differences of potential between the grid and the filament.

E Telephone circuit in which are produced oscillations by virtue of the variable coupling of the upper and lower inductances.

If we consider the filament to be hot, thus supplying free electrons, and the 220 volt supply to be sending a current through circuit *C* from the plate to the filament and through the lower inductance back through the lamps to the plate, there will be thus induced in the upper inductance a voltage which will create an alternating current in circuit *D*. This circuit

will have a frequency dependent upon the natural frequency of the circuit *D*, which may be evaluated as approximately $f = \frac{1}{2\pi\sqrt{LC}}$, in which *L* is the value in henrys of the upper inductance, and *C* is the capacity in farads present in the variable condenser. The frequency, *f*, is measured in cycles per sec.

The alternate charge and discharge of the condenser will cause changing differences of potential between the grid and the filament of the vacuum tube, and these changes produce in turn relatively large changes in the electronic flow in the plate current, *C*. These changes in the latter circuit again induce magnified voltages in the upper inductance. The cycle repeats itself, providing larger and larger currents in circuit *C*, until it effects a maximum of variation of the current in circuit *D*, where the "static" condition of an alternating current constant in its effective value has been reached.

This "steady" alternating current in circuit *D* induces a similar voltage in the lower inductance, which in turn sends it through circuit *E*. *E* thus has the same frequency as *D*.

The above summary represents the plan of apparatus necessary to produce a pure tone of constant pitch in the telephone receiver.[2] With the exception of the receiver itself, the apparatus is arranged on a rubber-covered table in *E*'s room. The receiver is isolated in the adjoining sound-proof room and is held constant in a standard. Further to insure the same position, the end containing the plate is inserted in a hole in the center of an upright lead sheet 14 by 14 in. This lead plate incidentally absorbs the sound waves which go back, and prevents reflection.

Method and Results

Briefly, the essential elements in the experiment are, first, to determine the amplitude of vibration of the telephone plate for some known current strength; secondly, to measure the current which produces a liminal intensity; and finally, to measure the energy in ergs which effects the liminal sensation in the ear for various pitches.

The physicist's definition makes of intensity that quantity of energy which passes in unit time through unit area of a surface placed at right angles to the direction of propagation of the sound. It depends primarily upon three factors: the amplitude and rate of vibration of the vibrating medium, the distance between the ear and the vibrating source, and the area of the vibrating source. The first two factors comprise the variables in the Wien-Rayleigh formula, which we use to establish the relation between strength of current and the intensity of the resulting tone.

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[2]The telephone used is one furnished by Kohl as part of Wien's instrument for measuring amplitude of vibration of the plate by means of a light wave reflected from a mirror on a lever. In the center of the plate is fastened a small steel rod to which we attached a glass rod for our linear calibrations.

$$A = \frac{C\,p_0}{2k} \cdot \Delta^2$$

$$\Delta = 0.147 \frac{k}{C^2} \cdot \frac{(2N)^2\ A\ R^2}{q}$$

A = energy per sec. passing through a square cm. of surface
C = rate of transmission of sound
p = own tone of the vibrating plate
k = index of specific heat
N = rate of tone (pitch)
R = radius of plate
Δ = relative pressure amplitude
q = distance between the ear and the plate

Before beginning the actual experiment, it was necessary to calibrate the different units of the apparatus to absolute terms. The amplitude of vibration for different current strengths was determined by attaching a very fine glass rod to the center of the plate with beeswax, and measuring its oscillations through a micrometer eye-piece. The current was read in terms of scale divisions on the string galvanometer with readings from an ordinary D'Arsonval and a rectifier of molybdenum used as a check for the lower tones.

Further calibration established the translation of the plate vibrations to galvanometer oscillations, and a consequent reduction of both to amplitude in mm. When these factors are known, it becomes relatively easy to determine the amplitude of vibration effected by the minimal intensities of various pitches; and, supplying this variable to the Wien formula, the total energy in ergs may be computed. A considerable amount of preliminary practice work was done with one standard tone of 120 vd., for which students from the elementary psychology classes acted as *Os*. While disparity in individual sensitivity was markedly apparent, an average of these measurements established the liminal energy for this tone in the region of 5.10^{-9} erg.

With the consequent addition of the tuning apparatus, more factors have become involved in the computation, but the method has remained essentially the same. The experiment lasts about one hour, with frequent rest intervals to prevent fatigue. The *O* is seated in the sound-proof room, the distance between the ear and the telephone plate being kept a selective constant by means of a head and mouth rest (sealing-wax biting board.) Reactions are transmitted to the apparatus room by means of a simple code of four responses made with a telegraph key and sounder, and representing the appearance, disappearance, decrease, and increase in the stimulus intensity. Verbal communication, though rarely necessary during the trials, is available by means of a rubber tube inserted through the wall.

The work of *E* involves the establishing of a desired pitch by combining certain units of capacity, and the adjusting of the inductance coils to provide a standard amplitude of current through the string galvanometer. The method of minimal changes has been used for the majority of the measurements, the limen being established, first, by subtracting units of resistance until the tone is heard; secondly, by adding them until it declines to inaudibility. In each step, some specific intensity is used as a constant to add to or to substract from. The determination of Weber's law follows the same general method.

With the high tones above 4,000, the amplitude of vibration becomes difficult to measure accurately, even in so delicate an instrument as the Edelmann galvanometer, and the readings are consequently recorded in

terms of unit resistance. For this purpose we have found the Leeds-Northrup rotating model to be of valuable assistance to ease and accuracy. In this series the current is left constant, and the telephone shunted around a resistance, the current through it being decreased by changing the resistance.

The bulk of the experiment was performed with 6 trained *Os*, all of whom were advanced students of psychology or assistants, including Miss Mary Palmer and Miss Sugi Mibai, graduate students, Mr. Richard Page, Mr. Adelbert Ford, Miss Edna Gordon, and Mr. Ernest Skaggs, assistants. Professor Pillsbury occasionally acted as observer, and was an indefatigable source of assistance in every phase and problem of the work.

The results of the work on these comparative limens are combined in Table B.

Prior to the addition of the tuning apparatus for different pitches, some preliminary work was done on Weber's law with a constant stimulus of 120 vd., the tone induced through the plate by a 60-cycle alternating current. The first method utilized a sliding rheostat for grading the intensity. Following the establishment of the individual limen, the tone intensity was increased or decreased by very slow manipulation of the rheostat; and whenever *O* reported an audible difference the current intensity was read in scale divisions on the string galvanometer and recorded. The averaged results from the 6 *Os* produce a general fraction of .2844, with a P.E. of .069.

In the second method, the telephone was placed in shunt with a Leeds-Northrup resistance box, and the tonal intensity was varied by passing the greater part of the current through the resistance. The readings in this series were recorded directly from the variations employed by *E*, and hence the table values read in ohms. Selecting first a minimal and than a maximal point, the limen was first established as in the preceding method. Resistance was then increased or decreased by steps of 1, 2 or 5, etc., the *O* reacting whenever he noticed a difference in the intensity of the tone in either direction. In this method, the shunt box was off the circuit, and the ear placed close to the telephone. The resulting fraction of .3152 is somewhat larger than that obtained by the first method, while its P.E. of .9422 is a trifle less.

In order to see whether different pitches would exhibit the same appreciable gradations of intensity, the establishing of certain limens was followed, where time permitted, by further experiments on Weber's law. The tones were selected from different portions of the scale, including particularly the areas surrounding the apparent turning points of intensity. In each instance, the limen as determined by previous experiment for a specific tone was used as the initial intensity or starting point.

TABLE B

Tone	Pr	Pg	M	Go	Fo	Sk	Wien
*d*120	5.10^{-9}	8.10^{-8}	2.10^{-7}	8.10^{-11}	1.10^{-9}	1.10^{-9}	
*d*341⅓	6.10^{-13}	3.10^{-11}	2.10^{-10}				
d 384	4.10^{-13}	9.10^{-12}	5.10^{-10}				
400							3.10^{-14}
d 426⅔	7.10^{-12}	6.10^{-11}					
c 512	5.10^{-13}	3.10^{-11}	3.10^{-10}				
d 576	3.10^{-12}	3.10^{-11}					
d 640	3.10^{-12}	2.10^{-11}	4.10^{-10}				
800							7.10^{-15}
c 960	3.10^{-12}	2.10^{-12}					
*d*1189	2.10^{-11}	9.10^{-11}	8.10^{-10}				
1600							1.10^{-15}
*d*1726⅔	1.10^{-13}	5.10^{-13}					
*c*2048	1.10^{-13}	4.10^{-12}	1.10^{-12}				
*c*2304	2.10^{-13}	4.10^{-12}	3.10^{-11}				
*c*2560	6.10^{-14}	6.10^{-14}					
*c*3072	2.10^{-14}	2.10^{-14}	5.10^{-12}				
*c*3200							5.10^{-16}
*c*3413¼	1.10^{-15}	5.10^{-14}	3.10^{-12}				
*c*3840	8.10^{-15}	3.10^{-13}	7.10^{-13}				
*c*4096	4.10^{-15}	7.10^{-14}	1.10^{-12}				
*c*5120	2.10^{-16}	6.10^{-13}	1.10^{-11}				
*c*6400	1.10^{-14}	8.10^{-14}	3.10^{-13}				3.10^{-15}
*c*8192	3.10^{-13}	5.10^{-13}	4.10^{-11}				
*c*9216	3.10^{-13}	6.10^{-13}					
*c*10240	6.10^{-13}	3.10^{-12}	9.10^{-11}				
*c*12288	9.10^{-11}	2.10^{-12}	3.10^{-11}				5.10^{-14}
*c*13650⅔	7.10^{-11}	8.10^{-9}					

With this as a basis, the O reported the first audible increase between this standard tone and a series of comparison tones of different intensity. This intensity then in turn became the starting point for a further increase, and so on up the scale. In the descending order, E gave the tone of maximal intensity as the starting stimulus, and O reported the first noticeable decrease in intensity, each subsequent determination becoming, in turn, a comparison basis for an ensuing weaker intensity.

A practical example may afford a clearer explanation of the specific measurements here. For instance, the liminal intensity for a tone of 512 vd. read, in terms of resistance, as 10 ohms. With this used as the initial intensity, E then decreased the resistance until O responded to a noticeable increase in the tone. Five such trials were made around this liminal value, and their average was accepted as the boundary of the first "step." The resistance was then set at this average, and this intensity was given as a standard or starting tone for the second step. The intensity was again increased until O designated his awareness of a change, the five repeated trials occurring here also as checks; and their average was accepted as the upper measure of the second step, or the lower standard intensity for determining the third increment. The procedure for the descending series of intensities was simply a reversal of direction, using this same method.

Table C includes the results on Weber's Law as hitherto determined. The fractional increment in this case represents an average for 14 ascending and 14 descending steps.

Translated into terms of intensity, the first step corresponds to a liminal sensation, while the maximal or fourteenth step provides a tone very easily audible throughout the room. The quantitative equivalents in ergs for these maximal points have not yet been computed, but it may be said in general that the fourteenth step is comparable in intensity to the average conversational tone of the speaking voice. Accepting this general hypothesis, and the apparent validity of Fechner's law, it seems safe to assume that the complete range of intensity gradations for the human ear would not exceed 100.

In these experiments, we have put into actual practice Fechner's suggestion for the measurement of sensations by using the just noticeable difference as the unit of comparison. Audition furnishes the ideal sense for these measurements because of the large fraction of Weber's law, and hence the small number of steps required. Our results indicate that there are but fourteen different steps, *i.e.*, fourteen different units, between the limen and a tone as loud as the ordinary speaking voice.

The acquisition of results from the louder regions of intensity has been somewhat curtailed by this method, owing to the 1000-ohm limit of the resistance box. It is highly desirable to extend the number of steps to as loud an intensity as can be practically obtained, and possibly this phase of the work will be carried on later in our laboratory. The results so far obtained, however, indicate that the principle of Weber's law does

TABLE C

	Palmer			Page			Mibai		
Rate	Limen	Fraction	P.E.	Limen	Fraction	P.E.	Limen	Fraction	P.E.
120	$5 \cdot 10^{-9}$	.4152	.0422	$8 \cdot 10^{-8}$	.3961	.031	$2 \cdot 10^{-7}$	.433	.0106
384	$4 \cdot 10^{-13}$	.2792	.0847	$9 \cdot 10^{-12}$	.2982	.074	$5 \cdot 10^{-10}$	.4012	.125
512	$5 \cdot 10^{-13}$	.26	.0276	$3 \cdot 10^{-11}$	.304	.0313	$3 \cdot 10^{-10}$	.418	.083
960	$3 \cdot 10^{-12}$	.3031	.008	$2 \cdot 10^{-12}$	.2054	.73			
1706⅔	$1 \cdot 10^{-13}$	.209	.0658	$5 \cdot 10^{-12}$	.293	.1037			
2048	$2 \cdot 10^{-13}$	.196	.0712	$4 \cdot 10^{-12}$	.2902	.081	$1 \cdot 10^{-12}$	.3011	.086
2560	$6 \cdot 10^{-14}$	.143	.106	$9 \cdot 10^{-8}$	.286	.008			
3413½	$7 \cdot 10^{-15}$	.273	.0296	$5 \cdot 10^{-14}$	.1832	.0915	$3 \cdot 10^{-12}$	.215	.0416
3840	$8 \cdot 10^{-15}$	.2021	.046	$3 \cdot 10^{-13}$	.2650	.993	$7 \cdot 10^{-13}$	.2974	.0642
4096	$4 \cdot 10^{-15}$	.1779	.0531	$7 \cdot 10^{-14}$	.201	.7261	$1 \cdot 10^{-12}$	.268	.072
6400	$1 \cdot 10^{-14}$	.1462	.114	$8 \cdot 10^{-14}$	.205	.05	$3 \cdot 10^{-13}$	.2739	.084
9216	$3 \cdot 10^{-13}$	.304	.091	$6 \cdot 10^{-13}$	.495	.137			
12288	$9 \cdot 10^{-13}$	.528	.1731	$2 \cdot 10^{-12}$	.5634	.1009	$3 \cdot 10^{-11}$	.6743	.017
13650⅔	$7 \cdot 10^{-11}$	.4794	.092	$8 \cdot 10^{-9}$	.8374	.209			

hold rather consistently for sound within certain limits, although the fraction is in no two instances identical, probably because of the errors of observation. The fraction near the limen shows consistently the lower deviation from the Law. With certain pitches, particularly in the lower range, it approaches one-half; in the medium range, it seems to maintain itself roughly at one-third or slightly less; in the upper range of the tones we used, around 12288 vd., it again approaches one-half or more.

In contrast to the conclusions of Smith and Bartlett, our data establish a slightly higher limen, in many cases, for the descending order of intensities than for the ascending, and the number of steps, when variable, is usually larger. No great divergence from these general tendencies was apparent in the results from different *Os*, the real contrasts occurring in the differential limens themselves and in different sensitivity to different pitches (Table B).

Table D

Table D includes fourteen steps from the ascending series of intensities for Miss Palmer. It serves to illustrate the variations in the fractional increments near and above the limen for tones selected from representative parts of the pitch range.

Steps	120 vd.	512	1706	2048	3840	6400	9216	12288
1	.468	.4325	.501	.4572	.571	.4218	.4273	.732
2	.4021	.3063	.4532	.396	.4032	.436	.561	.741
3	.4936	.491	.28	.4008	.401	.2997	.4821	.7064
4	.476	.3056	.278	.35	.312	.436	.4732	.623
5	.3495	.234	.2043	.2432	.2934	.171	.324	.544
6	.427	.2578	.3161	.2791	.2016	.203	.3041	.6921
7	.4241	.146	.2009	.268	.197	.1742	.3721	.5177
8	.305	.2932	.1937	.1104	.24	.2621	.209	.532
9	.3942	.205	.216	.3	.1911	.1287	.2573	.409
10	.401	.2005	.1867	.1873	.206	.1009	.3021	.5083
11	.43	.1893	.26	.29	.2173	.162	.256	.48
12	.372	.256	.1999	.19	.187	.2907	.305	.5112
13	.3589	.227	.254	.283	.272	.29	.49	.573
14	.47	.2436	.2126	.251	.33	.1894	.3783	.5805

Principal among the introspections from various *Os* was the observation that a maximal degree of attention during each experiment is necessary for valid judgments. The entrance of a fatigue factor always correlated with higher and more variable limens, and with enlarged and more uncertain increments of change. Careful consideration was, therefore, given to the *O*'s introspection of general feeling.

Some *Os* frequently reported auditory after-images and a difficulty in distinguishing the true minimal stimulus from subjective effects. This phenomenon apparently increased, rather than decreased, with practice, the higher sounds producing a more striking effect than the lower ones. In addition it may be

stated that the three *O*s having the lowest limens, and correspondingly the greatest acuity, encountered this difficulty most frequently. Two check-experiments with minimal values in intensity for 512 and 8192 vd., which employed the method of right and wrong cases, produced for them respectively an average of 78 and 81 correct perceptions, while for the *O*s less affected by auditory after-effects the general average of correct responses was 89%. The specific method in these two experiments was simply that of giving the warning signal 50% of the time when no current was on, and 50 % of the time when it was effecting a minimal intensity of vibration on the plate, and recording the percentage of correct and incorrect responses.

Kinaesthetic imagery seemed in general to predominate over visual, its effects occurring in a carrying-over of strain sensations of attention, and a feeling of effort to classify the different tones in some way.

The first three or four experiments for each *O* showed rather marked practice effects, but these did not appear subsequently in spite of repeated checks. The training seemed really to consist of increased attention and familiarity with reaction signals, rather than to involve any fundamental improvement in acuity.

One *O* reported difficulty in responding when the sound-proof room was illuminated, but it is probable that darkness merely mitigated a possible visual distraction for him.

The three lowest limens were secured from musically-trained *O*s; the higher limen and the larger fraction belong to an Oriental student with no musical experience; but the real significance of these factors cannot be definitely ascertained without further experimentation.

Conclusions

That tones of different pitch correlate with different sensitivity in the human ear is indicated by the difference in energy required to elicit response to their liminal stimuli.

Tones of the upper middle range are more easily perceived than tones either above or below it. Our results place these turning points respectively in the regions of 1000 and 6400 vd. Within these regions are apparent inconsistencies which illustrate differences in sensitivity both for certain tones and for the individual ear which reacts to the stimuli.

For Wien, the point of greatest sensitivity lies in the region of 3200 vd. Our results approximate 3483⅓ for two *O*s, and 3840 vd. for the third *O*.

Weber's law as applied to audition apparently holds true with a fraction of about one-third throughout the middle range of intensities. The fraction is larger for low tones and for very

high tones. The fraction is also larger near the limen, decreasing universally in the third, fourth, or fifth step. Whether this result is consistent through the upper range of intensities it has not been possible to determine until our apparatus is modified to produce greater intensities.

Kinaesthetic and auditory imagery are evidently predominant over visual imagery for all *Os* in the experiment, and there appears to be some correlation between auditory imagery and the limen of tonal acuity.

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MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by EDWIN G. BORING

XXV. THE EFFECT OF CHANGE OF INTENSITY UPON THE UPPER LIMIT OF HEARING

By E. F. MÖLLER

It has been suggested that the dependence of the upper tonal limen upon the intensity of the stimulus resembles the relationship that holds for the retinal color zones, where an increase in intensity of stimulus results in an extension of the zone such that a color of sufficient intensity may be recognized even at the periphery.[1] Savart[2] first observed that the upper tonal limen was different for different intensities of stimulus. Zwaardemaker[3] in 1893, and Scripture and Smith[4] in 1894, noted with the Galton whistle the general dependence of the limen upon intensity.[5] The latter concluded that "the general result for all observers indicates that the pitch of the highest audible tone varies directly and almost proportionately with the intensity." The Galton whistle has been subject to errors of calibration due to a failure to control the air-pressure during use and calibration,[6] but, although the absolute values of the limens of Scripture and Smith may therefore be doubted, the increase of the limen with increased air-pressure must probably be accepted, since changes in pitch of the whistle with overblowing would produce the opposite effect.

The present study is based upon experiments with steel acoustic cylinders of the Koenig type, actuated at various intensities by falling steel balls of different weights. Since the frequencies of the cylinders were calculated only, and not calibrated, the limens lack exact absolute meaning. The value of the study lies in the relationships of the relative frequencies as indicated by the psychometric functions obtained.

The stimuli employed were seven steel cylinders, selected from an octave, g^6—g^7, divided into twenty-one parts. This division did not give exactly equal musical intervals, since it was arranged to give all the diatonic intervals with the diatonic semi-tone (112 cents) divided into two, the minor tone (182 cents) into three, and the major tone (204 cents) into four parts. The scale thus consists of musical intervals of 56, 61, and 51 cents. The frequencies of the seven cylinders used (total range 341 cents; less than two whole tones) are shown in Table II.

It should be noted that these frequencies have been calculated by the manufacturers of the stimuli (Standard Scientific Co., New York), from a calibrated bar of greater length, on the theoretical assumption that the frequency varies inversely with the square root of the length. It would be desirable to calibrate the cylinders individually, but calibration is

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[1] *Cf*. E. B. Titchener, *Experimental Psychology*, 1905, II, ii, 40.
[2] F. Savart, *Ann. chim. phys.*, 44, 1830, 340.
[3] H. Zwaardemaker, *Zts. f. Ohrenheilk.*, 24, 1893, 304.
[4] E. W. Scripture and H. F. Smith, *Yale Studies*, ii, 1894, 111.
[5] In general, *cf*. Titchener, *op. cit.*, 40.
[6] *Cf*. Titchener, *op. cit.*, 32-36. Professor Ruckmick of Wellesley College will shortly publish a paper dealing with the calibration of the Galton whistle and certain related artifacts.

difficult. Koenig[7] noted that the calculated frequencies fall short of the calibration; the manufacturers of our cylinders claim to have obtained better results. For the present purpose, however, a knowledge of the exact frequencies does not matter except for the determination of the absolute position of the limen. The psychometric functions which represent relationships of sensory response to the stimulus-continuum are essentially the same so long as the stimuli represent a continuum and do not involve artificial inversions. There is no reason to believe that a series of cylinders, varying from one another only in length, give anything but a series of frequencies increasing regularly with a decrease of length. And indeed, in an experience with these cylinders in the Clark Laboratory which extends considerably beyond the limits of this experiment,[8] the fact that no inversions in qualitative psychometric functions have ever been obtained, excepting only for *B* in this experiment, indicated further indirectly that the cylinders constitute a continuous series of frequencies.[9]

The cylinders were suspended in a semicircular trough of 15 in. inside radius, of 20 in. outside radius, and 3 in. deep. The sides of the trough were padded with felt, the bottom with cotton covered by felt. The cylinders hung in a radial position, suspended by loops of dental floss each from a pair of metal strips that projected over the two edges of the trough. There was a space of 1 in. between the projecting ends of every pair of supporting strips, which left room for actuating the cylinders by a falling ball. Adjacent cylinders were 10.6° apart, *i. e.* about 4.5 in. on the average between the axes or about 3.75 in. on the average between the sides of the cylinders.

Vibration was secured by dropping a steel ball-bearing upon the cylinder from an electromagnet attached to a rotating radial arm. The magnet was centered over the center-line of the trough: thus the arm could be swung so as to allow the ball, when released from the magnet, to strike any desired cylinder. A small piece of rubber tubing at the end of the core of the magnet just kept the ball from actual contact with the steel of the core. Attached to the magnet was a small pointer, which indicated the position of the magnet on a scale fixed on each metal strip, thus controlling the exact point of impact of the ball with the cylinder.

The ball was required to strike a glancing blow upon the cylinder for the reason that it was likely to bounce and strike twice when the line of fall was the vertical diameter of the cylinder. The point of impact selected was the point of emergence of a diameter of the cylinder that makes an angle of about 4°52′ with the vertical diameter. Different intensities of stimulus were obtained by using balls of different weight as indicated in Table I. These balls were, of course, also of different size, and thus gave different heights of fall from the magnet, which remained at a fixed height. The necessary corrections were small, however, in comparison with the difference of weight, and it makes little difference whether these results are computed in terms of the weight or in terms of the energy (weight with variable height of fall taken into account).

In order to render stimuli of different sizes comparable, the arm had to be adjusted so that the point of impact, *i. e.*, the point of tangency be-

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[7] R. Koenig, *Wied. Ann.*, 69, 1899, 723.

[8] C. C. Pratt, this Journal, 31, 1920, 403-406; and in another unpublished experiment by the present writer.

[9] It is a question whether the stimuli in the method of constant stimuli need to be equally spaced, or indeed whether such a statement of stimulus-distances has any psychological meaning. The matter is wrapped up with the problem of mental units and of the logic of mental measurement in general and can not be gone into here. This paper gives some indication, however, of the manner in which relative results may have scientific meaning without regard to their absolute values.

tween the spherical surface of the ball and the surface of the cylinder, should remain the same. The lateral displacement of the line of fall for a ball of radius r_1 with respect to the line of fall for a ball of radius r_2 is given by the formula:

relative displacement $= \sin a\ (r_1 - r_2)$,

where $a = 4°52'$, the angle of the diameter of impact with the vertical diameter of the cylinder. The displacements of the last column of Table I are determined in this manner.

TABLE I.

Dimensions of the stimulus. Radius, weight, height of fall, and resultant energy of the 6 steel balls, dropped by electromagnetic release on the acoustic cylinders for different intensities of stimulus. The last column gives the lateral displacement of the line of fall from the perpendicular diameter of the cylinder necessary in order to secure impact at the same point on the cylinder (*i. e.* 4° 52′ from the vertical diameter).

Ball No.	Radius (cm.)	Weight (gm.)	Height of Fall (cm.)	Energy (g.cm.)	Displacement from Center of Cylinder (cm.)
1	.225	.435	7.999	3.479	.67
2	.300	1.040	7.849	8.163	.68
3	.375	2.025	7.699	15.590	.69
4	.475	3.505	7.500	26.287	.70
5	.550	5.546	7.350	40.763	.71
6	.625	8.315	7.201	59.876	.72

The relationship for the heights of fall, h_1 and h_2, of two balls of radii r_1 and r_2 is:

$h_1 = h_2 - (r_1 - r_2)\ (1 + \cos a)$.

The heights of fall in Table I are figured in this manner.

The total energy of the ball at the moment of impact is the product of the height of fall by the weight of the ball, but the energy effective for actuating the cylinder is that component normal to the surface of the cylinder at the point of impact. Since, however, the one is proportional to the other, it is enough to give the total energy, which is shown in the figures of Table I.

The observers were: Dr. C. C. Pratt (*P*), Mr. M. K. Macdonald (*M*), and Mr. F. L. Bixby (*B*), all highly practised in these judgments, since they had just completed observations on a similar problem, in which the same apparatus had been used.

The *O* was seated with his back to the apparatus and 4 ft. away. The instructions were read to him: "When the stimulus is presented, you are to say 'Yes', if you hear a tone, and 'No', if you hear no tone. Try to report immediately. Be sure of your judgment. If you are doubtful, ask to have the stimulus repeated. It is no matter how often you ask; never make a judgment when in doubt."

A few trials served to indicate a critical stimulus, which was then made the central stimulus in a series of five that included two cylinders on each side of the critical one. They were presented in haphazard order, with a 5-min. rest-period after each series of 100 judgments. A short preliminary warming-up practice preceded each series. One hundred judgments were taken on each cylinder for each intensity.

Table II shows the relative frequencies for the various calculated vibration rates and for the various intensities as indicated by the total energy of the falling ball. The rows of the table give the qualitative psychometric functions, which are plotted in Figg. 1-3, and the columns give intensive psychometric functions, which are plotted in Figg. 4-6.

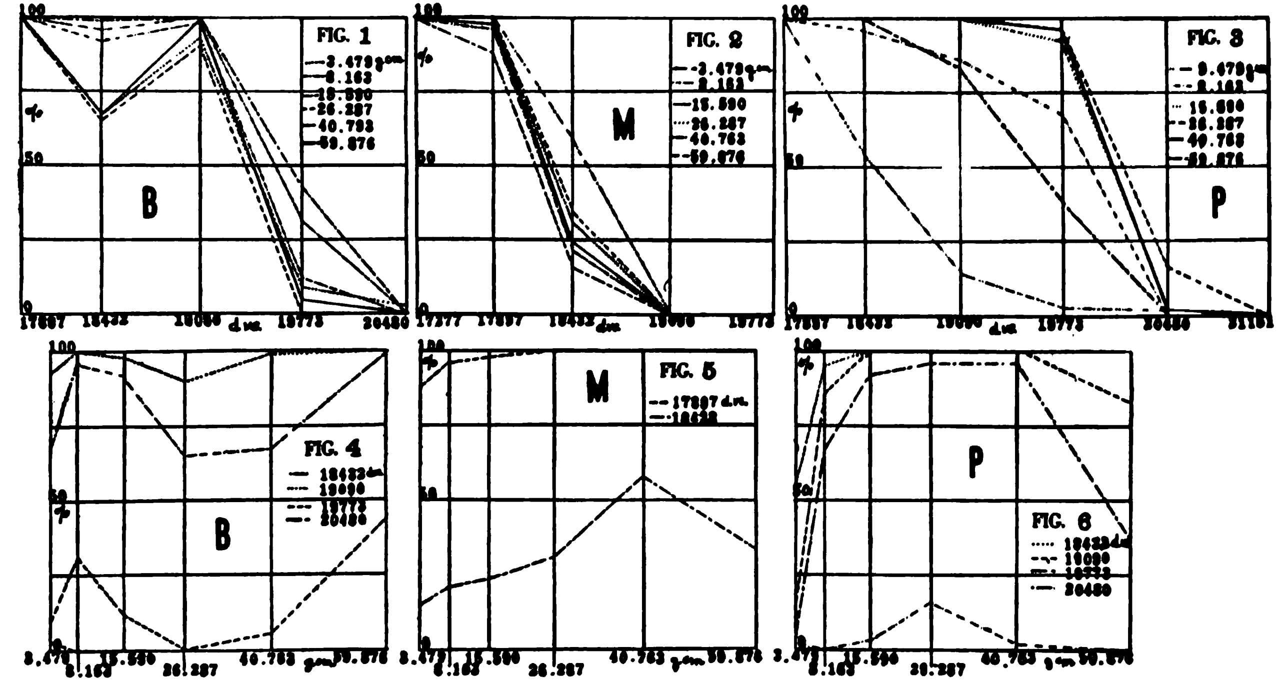
TABLE II

Observed relative frequencies of tones for various pitches (calculated vibration rates of the acoustic cylinders) and various intensities (energy of falling ball). The rows of the table give qualitative psychometric functions for the different intensities; the columns give intensive psychometric functions for different pitches. The 6 musical intervalls between the 7 stimuli in the table are successively from left to right: 51, 51, 61, 61, 61, and 56 cents.

Observer	Energy of Stimulus (g.cm.)	Calculated Vibration Rates of Stimuli (d.vs.)						
		17377	17897	18432	19090	19773	20480	21151
B	3.479		100	67	93	9	3	
	8.163		100	96	100	31	0	
	15.590		100	92	98	12	0	
	26.287		100	65	90	0	0	
	40.763		100	67	99	5	0	
	59.876		100	99	100	43	0	
M	3.479	100	88	15	0	0		
	8.163	100	96	21	0	0		
	15.590	100	98	24	0	0		
	26.287	100	100	31	0	0		
	40.763	100	100	58	0	0		
	59.876	100	100	34	0	0		
P	3.479		100	55	14	3	2	
	8.163		100	96	86	67	0	
	15.590		100	100	100	92	3	
	26.287			100	100	96	16	0
	40.763			100	100	96	2	0
	59.876		100	100	83	38	0	

In plotting the psychometric functions and in computing limens from them, we have used simply linear interpolation between the successive points. It is immediately evident from an inspection of the form of these psychometric functions that the *phi-gamma* hypothesis has no general validity for all psychometric functions: it is certainly not applicable here. We might have used Lagrange's formula as an indifferent hypothesis: a smooth curve that passes through all the observed points. A smooth function is probably more natural than a broken line, but, as Urban has shown, Lagrange's formula may lead to impossible interpolations, since it may give values above 100% and below 0%, and thus probably is equally in error in other regions. There is no particular justification for the straight line, except the practical one that it is easy to determine and renders interpolation easy. The work with Lagrange's formula is exceedingly laborious and there is no reason to believe that it gives any 'truer' result. It is good common sense, when we are equally ignorant of all hypotheses, to accept the least irksome.

By linear interpolation, then, we computed the terminal qualitative limens as a function of intensity. The results are shown in Table III and are charted in Fig. 7.



FIGG. 1-3. Qualitative (pitch) psychometric functions of tones for various intensities (energy of falling ball, g. cm.). FIGG. 4-6. Intensive psychometric functions of tones for various pitches (d. vs.). Observers *B*, *M*, and *P*. Graphic representation of the data of Table II.

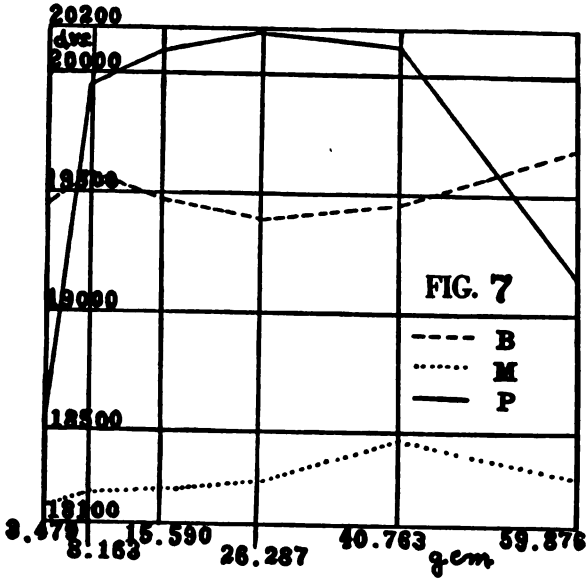


FIG. 7. Qualitative limens for various intensities of stimulus. Observers: *B*, *M*, and *P*. Data from Table III.

TABLE III

Qualitative limens, calculated from the rows of Table II by linear interpolation between the relative frequencies that include 50%.

Energy of Stimulus (g.cm.)	Observer		
	B	*M*	*P*
3.479	19,439.63	18,175.49	18,512.24
8.163	19,587.82	18,238.46	19,952.38
15.590	19,471.20	18,244.02	20,106.24
26.287	19,393.55	18,284.63	20,179.52
40.763	19,446.03	18,454.73	20,118.97
59.876	19,639.12	18,302.30	19,140.86

There are decided individual differences of qualitative terminal limen (Fig. 7). Except for the two extreme values of *P*, the individual differences are so much greater than the intensive variation for a single *O*, that the curves do not overlap.

The function that the qualitative limen is of intensity is different for different individuals. With increasing energy of stimulus, there is for *M* a decided rise followed by a decrease; for *B*, an increase, followed by a decrease, followed by a marked increase; for *P*, a great increase, followed by a great decrease (see Fig. 7). The form of the function is similar for *M* and *P*, but different in degree. *B*'s function is different from *M*'s and *P*'s, but similar in degree of variability to *M*'s. In no case are we able to say that the qualitative limen "varies directly and almost proportionately with the intensity."

A casual inspection of Fig. 7 might seem to indicate that the variation of the qualitative limen with intensity was insignificant and a matter of chance, perhaps of uncontrolled conditions. Inspection of Figg. 1-3 shows, however, that the relationships indicated by the form of curves in Figg. 7 hold consistently throughout the course of the psychometric functions. Fig. 7 is plotted for the limen defined as that value of stimulus most likely to give 50% positive judgments of tone. If, instead, the values of stimulus most likely to give other percentages are taken (*e. g.*, 75%, 25%), we find the same relationships holding. This fact is shown graphically in Figg. 1-3 by the fact that the various psychometric functions cross but rarely, that they have in general the same form, and that they lie in general in the same order throughout their courses. The argument for significant individual differences in these liminal functions is thus as follows: the differences cannot be an artifact of the cylinders, for the same relationship for a given *O* occurs with every stimulus capable of exhibiting difference; the difference cannot be an artifact of the different balls, for the same set of balls is involved for every cylinder for every *O*, and the different *Os* give different results; therefore the *Os* remain the only possible variant. It follows further that, if the differences in the course of the limen are significant, then the fact that the limen within these qualitative and intensive limits does not consistently increase with increase of intensity is also significantly established.

In general it is apparent that with material of this sort the mere statement of the limen gives but little of the available information. The interpreter of the data needs to keep the entire psychometric functions in mind if he is to have a complete knowledge of sensory response to stimulation at the upper limits of hearing. Especially does this fact appear in the case of *B*, Fig. 1. It will be seen that the relative frequencies for 18432 d. vs. are lower than for the stimuli on each side. One suspects at once a defect in calculation for the 18432 cylinder, but such an explanation will not hold. Neither *M* nor *P* shows an inversion at this point, nor did any inversions occur in extensive series taken in another experiment with these cylinders and with three other *Os* besides the *Os* of this experiment. Presumably therefore the group of psychometric functions may be taken as indicating for *B* a hypaesthetic region at 18432 d. vs. or else a hyperaesthetic region at 19090 d. vs. If the inversion in any of the psychometric functions had been great enough to cause the curve to cross the 50% abscissa, then it would have been possible to demonstrate statistically a tonal lacuna at 18432 d. vs. and a tonal island at 19090 d. vs., or, in psychophysical terms, a TR followed by an RL followed by a second TR. In a case of this sort it is apparent that no mere calculation of limens would ever give the total picture of auditory sensitivity. Even if we were willing to select some other frequency than 50% for the definition of the limen, we should not help ourselves, for there is no single abscissa that cuts more than three of the six psychometric functions, although all six functions demonstrate the same facts.

Figg. 4-6 show the intensive psychometric functions plotted from the columns of Table II. They represent the same facts taken from another aspect. Had the qualitative upper limen increased continuously with intensity, we should have been able to state the same fact by saying that the intensive lower limen (threshold) increased with pitch. In fact the psychometric functions of Table II are really not curves at all, but surfaces of relative frequencies plotted against pitch and intensity.

It is not possible in most cases to compute the intensive limens for the reason that a wide enough range of stimuli was not used. Two functions for *P* and one for *M* do cross the 50% abscissa. *P* would appear in this region to show a tendency toward an "intensive island." The function for 19773 d. vs. shows a lower limen at about 7 g. cm. and an upper limen

at about 56 g. cm. Between these two intensities, tone is heard more often than not. The other functions for *P* show the same general course, although they do not admit of the computation of limens.

There are individual differences among *Os* in the course of the intensive psychometric functions. *P* shows an increase followed by a decrease, both of an amount that is large in comparison with the qualitative change. *M* shows an increase followed by a decrease of amounts small with respect to the qualitative change. *B* shows consistently an increase, followed by a decrease, followed by an increase, all of a lesser degree than the amount of the qualitative change. It would thus appear that *B* has a region of intensive hypaesthesia followed by a region of intensive hyperaesthesia, that is to say, there are certain intensities which of themselves tend to increase the relative frequency with which tone is heard and which are more effective than are higher and lower intensities. The significance of these psychometric functions is indicated, as it was for quality, by the consistency among the functions for each *O*.

The degree of precision of the sensory response at these ranges of the tonal scale is indicated by the interquartile range of the qualitative psychometric functions, *i. e.* the change of pitch which would change the relative frequencies of the report of tone from 25% to 75%. In the eighteen qualitative psychometric functions, the interquartile range varies from 250 d. vs. to 830 d. vs., with an average of 494 d. vs. This average corresponds to a musical interval of about 40 cents, which is less than a quarter-tone, and shows a sensitiveness of discriminatory response not ordinarily expected in the region of the upper limit of hearing.

Conclusions

1. A complete account of sensitivity in the upper regions of hearing can not be given by the computation of limens; the complete psychometric functions must be considered.

2. Both qualitative and intensive psychometric functions can be determined simultaneously; the former indicates the qualitative upper limits of hearing, the latter the intensive lower limits for these qualities.

3. For any given *O* the qualitative psychometric functions for the different intensities are similar, and the intensive psychometric functions for the different pitches are similar.

4. There are individual differences in the forms of both the qualitative and the intensive psychometric functions. Both kinds of functions may show significant inversions or reversals and are not even approximately ogival in form.

5. Qualitative sensitivity, as indicated by the limen and also by the entire psychometric functions, does not, within the limits of this experiment and for these three *Os*, increase consistently with an increase in the energy of the stimulus, but follows a less simple law which varies for the individual *O*.

6. The qualitative psychometric functions indicate for one *O* the existence of a qualitative region that is hypaesthetic with respect to the next higher pitches. This phenomenon is presumably similar to the phenomenon of a tonal lacuna, but less extreme. Similar variations occur in the intensive psychometric functions.

7. The interquartile range of the qualitative psychometric functions is on the average about a quarter-tone, indicating an unexpected sensitiveness of discriminatory response.

XXVI. A Study of the Relation of Distracted Motor Performance to Performance in an Intelligence Test

By Miles A. Tinker

This investigation was undertaken with the hope of ascertaining whether there is any relation between motor performance under distraction to performance in an intelligence test. Often, at the end of an intelligence test, one hears the complaint that certain distractions, such as "nervousness" or the necessity for working against time, prevented the subject from doing his best. It was our intention to see whether these reports of "nervousness" or an objective test of distractibility would show a negative correlation with performance in the intelligence test.

Our results in the end were negative. No correlation was apparent, in part for the reason that the distractor instead of distracting proved a spur to attention. We are presenting the findings, nevertheless, because it is of value to have the outcome of such an attempt known, and because the results show in a striking manner the fact, already known in other contexts, that a distractor may prove an aid rather than a hindrance to many kinds of performance.

We had 39 subjects, of whom 33 were naive and 6 were members of the graduate department of experimental psychology.

All subjects were given the Otis Group Intelligence Scale; Advanced Examination, Form A.[1]

For the motor test for distracted performance we used two mazes, designed with a single univocal path without bifurcations in order to test speed and steadiness of motor performance but not learning. Each maze consisted of a path which wound from the periphery to the center of the maze and then out again according to the plan of the "walls of Troy." The total length of a maze is 72.5 in., and a maze covers a space of 11 by 9 in. The walls of the path were made of 0.25 by 0.25 in. brass strips, screwed to a wooden base covered with celluloid, with a path one-eighth in. wide left between. The two mazes were identical with the exception that one had notches one-sixteenth in. deep and one-eighth in. long, separated by one-eighth in., all along the walls of the path. The notches in one wall were directly opposite those in the opposing wall. A metal stylus, one-sixteenth in. in diameter, was used to trace the path. The stylus and the maze were connected electrically with markers on a kymograph so that a graphic record of all contacts of the stylus with the walls of the maze was obtained. A time-line on the record gave the time for traversing the maze. At *S*'s ear on top of a post was a cigar box, for a resonator, with an electric bell attached. The bell was wired so that, when a switch was closed, it would ring every time the stylus came in contact with the wall of the maze.

The notched maze was intended to place a greater emphasis upon accuracy as against speed, since *S* might catch the stylus in the notches if he hurried. As a matter of fact, however, the notching reduced accuracy as well as speed, so that the introduction of the notched maze did not enable us to reduce accuracy and speed to a single variable as we had hoped to do.

The ringing of the bell close to *S*'s head when a contact was made was intended to be a distraction and to induce "nervousness."

[1] *Jour. of Educ. Psychol.*, 1918, 9, 239-261; 333-348.

Procedure

The intelligence test was given according to the directions in the Manual[3] except for the following reductions in the time allowed for certain of the tests: test 4, from 6 to 4 min.; test 5, from 6 to 5 min.; test 7, from 3 to 2.5 min.; test 8, from 4 to 3 min.; test 9, from 6 to 4 min.; test 10, from 3 to 2 min. We made this change because we have found from experience that, if the full time is given, too many finish before the time is up.

At the end of the test the *S*s were asked to answer certain questions intended to reveal the degree of "nervousness" which they experienced in taking the test. The following directions were read to *S*:

"This is a test of steadiness. You are to take the stylus and try to trace the path through the maze without toucing the sides. The problem is to get through in the shortest possible time with the least number of touches. Every time you touch the side the error is recorded electrically and counts against your score."

The order of tracings in the two mazes was as follows:

1. Smooth.
2. Notched
3. Notched
4. Smooth

} No distraction

5. Smooth Unexpected distraction

6. Smooth
7. Notched
8. Notched
9. Smooth

} Expected distraction

Before the fifth tracing the switch which connected the bell was closed without the knowledge of *S* and the bell rang every time a contact was made. Before the sixth tracing *S* was warned that the bell was still attached and would ring every time that he touched the wall of the path with the stylus.

At the end of the tracings questions were again asked in order to bring out *S*'s opinion of his degree of "nervousness" in performing the test.

Results

Averages for the results of the motor performance test and of the intelligence test are given in Table 1, and the significance of some of the differences between these averages is indicated in Table 2.

In the first four rows of Table 1, if we compare the tracings in the smooth and notched mazes for no distraction and for the expected distraction, we find that both the time of tracing the maze and the number of contacts vary in the same direction and in approximately the same degree. For this reason we felt justified in combining the results for a distractor and for no distractor in the fifth and sixth rows of the table, in order to show the general difference between the smooth and notched mazes. Originally we had introduced the notched maze with the intention of rendering a rapid traverse of the maze difficult and thus of placing a premium upon accuracy as against speed. We had thought in this manner to obtain some comprehension of the supposedly inverse relationship between speed and accuracy, and thus to be able to reduce the two measures of performance to one. We find, however, that the relation is not inverse, for the introduction of the notches not only lengthens the time but also decreases accuracy (in spite of the fact that the notches reduce the probability of contact since the path is wider between opposing notches). It is apparent

[3] *Otis Group Intelligence Scale, Manual of Directions*, 1920.

therefore that we must in this case regard speed and accuracy in the maze as at least partially independent variables, since they vary together and not inversely when the change is made from the smooth to the notched maze.

It appears also from Table 1 that the effect of the introduction of the expected distractor is similar in direction and amount for both the smooth and the notched maze (see the first four rows). Accordingly we have combined these results in the seventh and eighth rows so as to show the general effect of the introduction of a distractor. It is here evident that the presence of the distractor decreases the speed and increases the accuracy. Such a result is equivocal, for the reason that we can not tell whether the decrease of speed is responsible, at least in part, for the increase in accuracy. We have seen that speed and accuracy may vary independently as they do when notches are introduced in the walls of the maze; it is natural therefore to regard them as independent here. The argument for their independence is, moreover, considerably strengthened by the results from the introduction of the "sudden distractor," *i. e.*, the distraction begun without warning to *S*. In the ninth row of Table 1 we see that speed was not appreciably changed by the distraction, whereas the accuracy was greatly increased. Certainly, then, the variation must have been independent in this case, and it seems highly probable that it is to be so considered when the expected distractor is compared with the normal case.

What happened must have been approximately as follows. In the first four trials the *Ss* settled down to a given degree of speed and accuracy for each of the two types of mazes. In the fifth trial they were startled into a higher degree of attention by the unexpected ringing of the bell whenever an error was made. The average speed of the preceding trials was maintained approximately, but the "distractor" acted as a spur to attention and the *Ss* consequently worked with much greater precision. In the subsequent trials where the distractor was continued they may have been fatigued: the *Ss* slowed down and became less accurate than with the sudden distraction; they remained, however, more accurate than in the initial undistracted trials, either because of the attentive spur of the distraction or because of the slower rate. That intended distractors, especially when intermittent, may fail to distract and may instead act as a spur to attention and thus lead to intensification of impression or to shortened reaction time is well known.[3] In our experiment some of the *Ss* commented upon the steadying effect of the ringing of the bell, and also upon the advantage that it gave them in notifying them when they made a contact. On the other hand many *Ss*, who reported that the test had made them "nervous," stated that the bell made them especially nervous. It is not impossible that the bell not only made the *Ss* "nervous," but also spurred them on to better work, and that in general the conditions of "nervousness" in such a test—and perhaps also in an intelligence test—may also be the conditions of accurate performance.

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[3] H. Münsterberg and N. Kazaki, The Intensifying Effect of Attention, *Psychol. Rev.*, 1894, 1, 39-44; A. J. Hamlin, Attention and Distraction, this JOURNAL, 1896, 8, 3-66; J. E. Evans, The Effect of Distraction on Reaction Time, *Arch. of Psychol.*, 1916, no. 37, vol. 5, 1-53; E. E. Cassell and K. M. Dallenbach, The Effect of Auditory Distraction upon the Sensory Reaction, this JOURNAL, 1918, 29, 129-143.

TABLE 1. Average performance in speed and accuracy in motor test of steadiness. Figures are averages for time (secs.) and number of contacts with the walls of the maze for 39 subjects. The numbers in the second column are series numbers. Performance on the Otis intelligence test is shown in the last two rows.

		Time		Contact	
		Av.	M. V.	Av.	M. V.
No distraction	Smooth maze: 1, 4	76.4	21.2	74.5	15.6
	Notched maze: 2, 3	86.0	20.2	109.8	24.6
Expected distraction	Smooth maze: 6, 9	82.5	30.3	61.6	15.2
	Notched maze: 7, 8	92.5	25.9	93.9	21.2
Smooth maze	1, 4, 6, 9	79.8	20.0	67.9	13.1
Notched maze	2, 3, 7, 8	91.6	21.4	100.4	21.1
No distraction	1, 2, 3, 4	81.1	18.5	90.3	15.5
Expected distraction	6, 7, 8, 9	90.5	27.2	79.8	14.0
Sudden distraction	5	82.4	33.2	69.2	16.5
Otis test	Score	155.5	26.2		
	% accur.	.859	.070		

Table 2. Significance of differences between averages of Table 1. The table shows the significance of the difference found in changing from the smooth to the notched maze, and in introducing a sudden or an expected distractor during the performance. D/P.E.$_D$, the ratio of the difference to its probable error, is the usual measure of significance; P_D, the probability of difference, is, on the assumption of the normal law, the probability that the difference will not vary from itself by an amount more than itself.

		$\frac{D}{P.E._D}$	P_D
Smooth *vs.* notched maze	Time	2.98	.956
	Contacts	9.67	1.000
Sudden *vs.* expected distraction	Time	1.39	.651
	Contacts	3.63	.986
No distraction *vs.* expected distraction	Time	2.11	.845
	Contacts	3.73	.988

Table 3 gives the results which the problem was planned to educe. It was hoped that change in performance in the maze under the introduction of a distractor would prove an objective measure of distractibility

or "nervousness." A significantly high negative correlation of this change with performance in the Otis test might indicate that such distractibility as an individual characteristic was a special disadvantage in an intelligence test; whereas a high positive correlation might have meant that the distractor acted as an attentive spur and that "nervousness," so defined, was of advantage in taking an intelligence test. As a matter of fact, however, the correlations are all low. Since speed and accuracy seem to be independent variables in the maze, they had to be treated separately, and the effect of the one or the other eliminated by partial correlation. In the same manner score and accuracy on the Otis test were separately cared for. The last column of Table 3 shows, however, that distractibility neither as measured by speed (T) nor as measured by accuracy (C) is highly or significantly correlated with either of the measures of performance on the Otis test, even when correction is made by partial correlation for variation of the two factors not entering into the correlation.

TABLE 3. Coefficients of correlation (products-moments method) and partial coefficients of correlation between T, C, S, and A, defined as below. The probable errors of these coefficients are all between .06 and .10.

T = change in time for traversing maze when expected distractor is introduced.

C = change in number of contacts made in traversing maze when expected distractor is introduced.

S = score in the Otis intelligence test.

A = accuracy (ratio of items right to items attempted) in the Otis intelligence test.

$r_{TC} = -.37$	$r_{TC.S} = -.39$	$r_{TC.A} = -.39$	$r_{TC.SA} = -.39$
$r_{TS} = -.08$	$r_{TS.C} = -.16$	$r_{TS.A} = -.10$	$r_{TS.CA} = .07$
$r_{TA} = -.10$	$r_{TA.C} = -.18$	$r_{TA.S} = -.06$	$r_{TA.CS} = -.13$
$r_{CS} = -.17$	$r_{CS.T} = -.21$	$r_{CS.A} = -.08$	$r_{CS.TA} = -.13$
$r_{CA} = -.17$	$r_{CA.T} = -.21$	$r_{CA.S} = -.08$	$r_{CA.TS} = -.13$
$r_{SA} = .67$	$r_{SA.T} = .67$	$r_{SA.C} = .67$	$r_{SA.TC} = .67$

There is little to be gained from the results of the questionaries given the *Ss* after the intelligence test and after the maze trials. The *Ss* tended to follow the suggestion of the questions and to admit "nervousness." There were 21 who reported themselves "nervous" in both the intelligence and the maze tests; 6 who admitted "nervousness" in neither; and 12 who were "nervous" in one but not in the other.

Conclusion

At first sight our results seem mainly negative. The introduction of an auditory distraction during a test of motor steadiness may not distract the *S*, even though he reports a conscious disturbance, but may spur him on to more accurate manual performance. The distractor has a measurable effect, howbeit in an unanticipated direction. The sensibility of an individual to this sort of effect is not, however, prognostic of his performance in an intelligence test.

There is, however, an application of a known psychological fact which the experiment renders the service of indicating. We have known that

sensory impression may be reenforced attentionally by the facilitating effect of an intended distractor that does not distract, and that reaction times may be shortened in the same manner. We have now shown that manual precision of movement may similarly be increased by the concomitance of an intermittent intended distractor. Obviously the next experiment is to make the analogous direct attack upon the intelligence test itself. May it not be that the presence of a distractor will improve the performance of a group in an intelligence test? At any rate we know that complaints about distraction have in themselves little value as bearing upon the distracted performance. It may be unpleasant to be "distracted;" it is generally unpleasant to have the attention spurred; but, pleasant or unpleasant, the spur may result in improved performance for the individual and thus justify itself in spite of the contrary opinion of the *S*. Even if an apparent distraction does not improve intelligent performance, we have certainly no way of knowing whether it interferes. The direct experimental attack needs yet to be made.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXI. The Areal and Punctiform Integration of Warmth and Pressure

By Ida Bershansky

This study was undertaken in the hope of resolving a difference in the results of recent investigations of the integration of warmth and pressure. Malmud had found only a close fusion of the two qualities, described at times as a warm-pressure, and at times as a pressury-warmth.[1] Cobbey and Sullivan, on the other hand, had reported a perceptive integration which they called 'oiliness'.[2] There was, however, a difference in method of attack. Malmud began her experiments by arousing simultaneously punctiform warmth and pressure, and her *Os* were asked to report the course of the resulting experience; she assumed that, if a perception appeared, it would be indicated. Cobbey and Sullivan first employed areal stimulation with the intent of determining the 'compulsory conditions of the oily perception', and then, after this perception was known, undertook a punctiform stimulation. They suggest that Malmud's negative finding might be explained by the difference in procedure; and we have, therefore, repeated their experiment.

Our experiment, like its original, was divided into two parts. In the first, we immersed the first joint of a finger in some oil or in warm water of known temperature, and we asked our *Os* to report the resulting sensory experience, particularly as regards the first impression, and then as regards any changes that might take place in course; they were also instructed to name the perception. In preliminary experiments we employed two *Os*, Miss G. Adams (*A*), and Miss M. E. Smith (*S*), both of whom were experienced in psychological observation; later we added as a third *O* Assistant Professor Hoisington (*H*). The *Os* were blindfolded and their nostrils were stopped with cotton-wool before they entered the experimental room. They sat with the right arm placed on an arm-rest which extended over the edge of a table, and which contained a hole through which a finger was thrust; this served to keep the arm and finger in a fixed position. The stimuli used were kerosene oil, olive oil, water, and castor oil, which were presented in the order named. The water was warmed by means of an electric coil; at the beginning of an experiment the temperature was 32° C., and during an obeervaion it was gradually increased to 38° C. or 40° C. The average temperature of the oils and of the experimental room was 21° and 20° C. respectively.

The procedure in a single experiment was as follows: *E* gave the signal 'now'; and then, slowly and evenly, by means of a mechanical device, raised a small glass vessel filled with water or oil, as the case might be, until the first joint of the finger was immersed; and *O* immediately began his report. The stimulation was continued for 10 min. unless *O* reported fatigue or heat. In order to remove any clue to the nature of the stimulus-object after the period of stimulation the finger was dipped in gasoline; and at the close of the experimental period the finger was washed with soap and water before the blindfold and the cotton-wool were removed.

[1] R. S. Malmud, this Journal, xxxii, 1921, 571.

[2] L. W. Cobbey and A. H. Sullivan, this Journal, xxxiii, 1922, 121.

In these preliminary experiments *S* in 38 trials reported 12 perceptions, 3 when kerosene, 1 when water, 3 when olive oil, and 5 when castor oil was the stimulus; *A* in 40 trials reported only one perception of an object. Characterizations of these perceptions are:

S. *Olive oil*. 'Just like putting the finger in fine flour; it is an oily smooth, a sort of thin oil;' 'tiny indication of oiliness like cold cream on the hands when you have not wiped it all off.' *Castor oil*. 'It feels like warm thin butter;' 'there is a little coolness in the pressure, it may be an oily damp; now it is a little greasier than anything else, it is a sort of combination of cool wet and grease;' 'there is just a little pressure and cool, an oily damp like sticking finger into lard.' *Kerosene*. 'Feels like putting the finger in cold oil; it is not a watery wet, but a smooth wet. It is a cool and smooth pressure, an oily damp.'

Warm water. 'It is a nice smooth warm; a thinner feeling of pressure than the oily feeling; the other feels more oily, this feels thinner.'

A. *Kerosene*. 'Just as if I had been touched by warm jelly; I cannot say what it was because it wasn't a familiar perception.' For the rest this *O* employed such terms as 'close pressure', 'soft warm pressure', 'a pressure which sticks close around my finger', etc.

At this stage of the experiment we were puzzled by the fact that *A* reported no more perceptions, and by the further fact that *S* with a single exception failed to get the perception with warm water. A study of the reports led us to believe that the contrast between the temperature of the oils and that of the water was a disturbing factor; and, since fatigue and adaptation appeared in nearly every experiment, we decided also that the period of stimulation was too long. We therefore raised the temperature of the oils to 30° before stimulation, lowered the initial temperature of the water to the same point, and resolved in subsequent experiments to remove the stimulus as soon as *O* had reported a perception. With these changes in method, and with *H* as an additional *O*, we began another series.

In this second set of experiments *S* reported 10 perceptions in 24; *A*, 2 in 28; and *H*, 25 in 31 trials. Perceptions for all *Os* are as frequent with warm water as with any other stimulus, and also with the thinner as with the heavier oils. The characterizations of the perceptions of *S* were as before. We tried to force the perceptive attitude upon *A* by asking her every time to name the perception; but she was still unable to do this; she repeatedly reported the experince as wholly unfamiliar; and the most that she could say was, for example: "I feel something soft and warm and something heavy closing in around my finger; it felt soft, but I did not get the perception of any object;" "I get some kind of perception but it isn't anything that I know; it felt something like squeezing the finger with a rubber glove." The one other instance of a perception of object was: "It was just as if I were touched by a warm jelly; I cannot say what it was; it wasn't a familiar perception." Typical perceptions of *H* are as follows: "I cannot objectify the perception because there is nothing like it in my experience;" "I don't know what to call the perception, but it is a snug cozy something that has considerable density, and lies tightly around the finger like oil;" "it is like a semi-liquid or a heavy oil only I do not get the smoothness as when I rub my fingers together;' "it is like a dense liquid that clings closely to the finger. It might not be a liquid; I am using the term inferentially; I cannot conceive of anything as acting on my finger like this except a liquid;" "a clinging oily liquid that fluctuates in density as the warmth fluctuates in intensity;" "it is like thin butter;" "like warm molasses without stickiness, or a heavy oil that was thick."

Psychologically, our *Os* have described the experience as a fusion of warmth and pressure which may be characterized as a "warm snug pressure" or as a "warmth that sticks close to the finger." In this respect there

seems to be complete agreement with the *Os* of Cobbey and Sullivan. All these, however, named the perception 'oiliness;' whereas our *Os* were not satisfied either as individuals or as a group with any one term as a name for the perception. *S* and *H*, it is true, employed the word 'oiliness' more often than any other; and they named objects that were oily more frequently than any others; but they also named objects that are not oily, such as glue, mercury, jelly, rubber glove, and molasses. If, therefore, we accept the term 'oiliness,' it must be with some reservation. It is well known that the common perception of oiliness involves movement. In our experiment every effort was made to prevent movement of the finger during the period of stimulation; and *H* says, as we have seen, that the perception under these conditons is "like a semi-liquid or heavy oil, only I do not get the smoothness as when I rub my fingers together." Furthermore, at the close of the experiment he states: "The perception is one that we get from liquids, including oils; it is just as much like mercury or molasses without the stickiness as like oil."[3]

A by-product of the experiment which is of psychological interest is the course of the perception with change in degree of temperature. If the initial temperature of the water (30°) was below that of the skin, the perception was almost invariably of 'wetness.' Following this, as the temperature increased, the perception changed first to a soft, snug warm pressure which meant a thin (sometimes oily) liquid; and then, with still higher temperature, to a closer, tighter, warm pressure which carried the meaning of a thicker, denser, more viscous liquid. The higher limit in intensity was about 36°; beyond this, the warmth became dominant in intensity and clearness, and the fusion was broken up; the warmth was then felt as a 'radiant warmth with a background of pressure.' The effect of the increase in warmth was to increase the intensity, *i. e.*, the closeness, snugness of the pressure. It will be noted that, under the conditions of our experiment, pressure is felt either over the entire surface or over patches of the skin *below the surface of the liquid stimulus*. The ring of the pressure gradient is felt, if at all, only when the temperature of the liquid is approximately that of the skin;[4] the *warmth* is felt as warm-pressure, and increase of warmth as increase in the intensity of warmth and pressure.[5]

In the second part of the experiment we employed a punctiform stimulation of warm and pressure spots. For the pressure stimulus we obtained excellent results by Cobbey and Sullivan's method of raising a hair to the vertical position; better still, by bending the hair backward. For the

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[3]In the hope of furnishing a perception that would serve as a slight contrast to that of our experiments, and that might, therefore, aid in characterization, we occasionally and without warning exchanged the usual stimulus for one of flour warmed to a temperature of 37°. *S* reported the object of perception as 'flour or some powder;' *A*, as a granular, resisting substance; and *H*, as a dense, semi-liquid substance with a density like mercury and a clingingness more like oil.

[4]There were times when *O* did not know when his finger entered the liquid (water or kerosene), and there were also times when he got no experience while his finger was in the liquid. A ring of heat is sometimes felt when the temperature of the water is above 38°.

[5]It is, of course, possible that the pressure in this case may have its origin internally, and be referred to the surface. Internal pressure is, however, frequently reported, and we should still have to explain why increase in warmth of stimulus carries with it increase in felt pressure.

warmth we were most successful with Dimmick's electric stimulator. Our procedure was as follows. We first isolated warm spots which had a hair within their area or very near them. Then, in an experiment, we first aroused warmth; and when it was reported, raised the hair. Under these conditions we obtained a perception in 7 of 11 trials with *H*, in 5 of 24 with *S*, and in 1 of 35 trials with *A*. The naming of the perception was found to be much more difficult than in the areal experiments. Typical reports are: (*H*) "Qualitatively, I do not see much difference between this semi-liquid and the one my finger was in;" "I get the warmth and pressure beaten up in perception, it was oily in quality;" "the experience is like a drop of dense liquid; it is not wet; I don't know what to call it; it is a little like dense mercury, and a little like a heavy oil;" (*S*) "It is oily rather than wet;" "when the warmth and pressure are nearly equal I get the perception of oiliness;" "it feels a little like warm butter on the end of a toothpick. The perception does not come easily; it is hard to name it." The one perception of *A* was: "It feels sticky, like grease."

Psychologically, the experience is nearly always a warmish pressure; it is a fusion in the sense that it is unitary and yet may at any time be analysed into the two qualities. In the integration the pressure, which must be steady and not too intensive, seems to spread a little and to lose its sharpness of definition.

Conclusions. Our results appear to explain the divergence noted at the outset of this Study. There can be no doubt that the warmth-pressure integration, if it suggests an object of perception at all, most often and most naturally carries the meaning of oiliness. If, then, the *Os* are set for a one-to-one correlation of experience and perceptive meaning (and this set may be induced without any corresponding instruction from *E*), they will give a regular report of 'oily' in the synthetic experiment. In so far we agree with Cobbey and Sullivan.

But oiliness is not, under ordinary circumstances, a sheerly cutaneous perception. If, then, the *Os* are not set for perceptive report, the word 'oiliness' need never occur to them; and if they are set for perception generically only, and not specifically, they may vary in their reports,—they may fail to discover an appropriate term, or they may interchange 'oil' with such other substances as 'butter' and 'glue' and 'molasses,' or they may settle easily upon the single word 'oily.' In this way we account for the results of Malmud and for those of our present observers.

It seems, therefore, more nearly true to say that the integration of pressure and warmth is a compulsory part-condition of the perception of oiliness than to regard it as the single adequate condition.

LXII. The Integration of Warmth and Pain

By Lucile Knight

This study forms a member of a series undertaken to discover what results psychologically when warm, cold, pressure, and pain spots are taken in pairs and stimulated simultaneously.[1] In this investigation we have worked with warm and pain spots.

[1] For references to the earlier investigations, see R. S. Malmud, this Journal, xxxii, 1921, 571. See also J. H. Alston, *ib.*, xxxi, 1920, 303; and L. W. Cobbey and A. H. Sullivan, *ib.*, xxxiii, 1922, 121.

Before beginning the experiment proper we practised the technique of localization and stimulation of pain spots, and acquainted our *Os* with the qualities of pain. All spots were localized within an area 2 cm. sq., on the volar side of the fore-arm: the skin was first softened with soap and water, and then explored with a sharpened horse-hair 2.5 cm. in length. This stimulus was found to be inadequate to the arousal of pain when the skin was etherized. After two weeks of preliminary work we localized warm and pressure spots within the same area, and selected for experimentation spots that responded with a warmth of good intensity, and that had closelv neighboring pain spots. We took particular care that no pressure spot was near enough to be stimulated in any way during the course of an experiment. In a first series of experiments we employed two *Os*, Miss E. Powell (*P*), and Miss E. Davis (*D*), both of whom were specializing in psychology. For the arousal of warm spots we used Dimmick's electrical apparatus, which stimulates the spots by radiant heat. This was so mounted that it could be lowered to a position about 2 mm. from the surface of the skin, and then, when warmth had appeared, could gradually be raised and the warmth still maintained. Our procedure in a single experiment was to arouse warmth; then, when it was reported, to give the signal 'now;' and after 1.5 sec. to stimulate the pain spot (which had been softened with vaseline) with the sharpened hair. The instruction to the *O* was as follows: "You will be given a cutaneous stimulation on the forearm. Report the course of the resulting experience. You may give a running account if you like."

The results obtained by this method were not satisfactory; the heat from the apparatus frequently not only curled and therefore destroyed the hair, but also dried the skin so rapidly that pain spots failed to respond. Both *Os*, however, gave reports which indicated that the conditions of an integration were at times obtained. The two qualities seemed either to form a spatial pattern in which they ran their courses side by side, or to fuse in a new quality which was called 'hotness'. From a study of the reports we felt moderately sure of this fusion; but we could not be certain that the 'hotness' was not occasioned by the radiant heat of our apparatus. *E*, it is true, always raised the apparatus to what she thought was a safe distance; but she had no other guide than the reports of her *Os*. We determined, therefore, upon a test experiment that should more adequately be controlled, and that should be undertaken with more experienced *Os*.

In this second series of experiments we mounted the electrical apparatus on a universal standard and let *O* himself, by turning the screw which raised or lowered the stimulator, control the degree of warmth. We also placed the arm in a plaster cast, took the additional precaution of selecting warm spots that were not contiguous to cold spots, and aroused pain by touching the skin lightly with a needle. The *Os* were Professor Weld (*W*), Professor Hoisington (*H*), and Dr. Bishop (*B*). The author also observed in a few experiments in which Professor Weld served as *E*. *O* was asked to adjust the apparatus until he felt a continuous warmth, and then to say 'now;' the pain stimulus was then applied, and *O* straightway described the resulting experience.

Under these conditions 'burning heat' or 'hotness' was usually reported. In general, the loose, comfortable warmth contracts about a hard penetrating stingy pain; the warmth is replaced by a definitely painful sting (heat). We were able to compare the fusion thus obtained with an actual burn by lowering the apparatus until burn appeared, then raising it until only warmth remained, and then stimulating with the needle. At first all *Os* were unable to distinguish the two experiences, but after a few trials an extensive difference was made out. Typical reports of the *Os* are as follows.

H. "The latter [with the needle] is a little less extended; qualitatively, I don't see any difference;" "a sharp one this time; it was more

limited in extent but quite as penetrating and as burny;" "I called 'now' when the other sting was there, so I had the two together. The sting from the heat was weak, that of the needle more intense and went deeper; qualitatively I could not say which was which. In general, the difference is one in intensity; the one with the needle is usually less extended, although they may be similar in extent; the actual burn is more like several points, but they are not so lively."

W. "The needle-burn was much more pointed, sharper, not so sting-like this time, but smaller and more punctiform. Qualitatively, the two experiences are exactly alike; the difference is areal;" "the warmth was there, a deep diffuse warmth, and then it changed to a warmth centralized, punctiform and more like pain; the change was not sharp or sudden like a typical prick; it was more durative, like an increase in intensity."

B. "Burning warmth again! It differs from the actual burn in that it is brighter, more concentrated, smaller in area. It seems centered in an area of soft warmth, a little hard part that creeps in;" "except for an occasional dull pressure that comes in with the needle prick, the two experiences are qualitatively the same; the sting is less intensive than with full heat, *i. e.*, the burn is not so great."

All *O*s agree that the experience is a fusion, probably of warmth and prick, but it may be of sting (heat) and prick. At times it is a warm painful sting, at others a merely painful sting. In the latter case the warmth seems to become stingy, and this invariable stingy quality makes analysis difficult; it is not easy at times to distinguish it from warmth, and at others from pain. We are accustomed to regard sting as an intermediate quality which results from the simultaneous stimulation of warm and cold spots; but it is reasonably certain that no cold spots were stimulated in the present experiments. We found also that, with continuous stimulation of a warm spot by radiant heat, warmth changes to sting before burn appears; and in a few casual observations we were unable to distinguish this sting from that obtained by the simultaneous stimulation of a warm and a cold spot. The point would seem to be of considerable systematic importance, and we regret that we have been unable for lack of time to pursue it further.[4]

We are convinced, however, that warmth, particularly a warmth of good intensity, is qualitatively much more like sting than Cutolo has led us to suppose.[5] In our attempt to obtain an analysis of 'burn' we repeatedly asked our *O*s to compare the warmth and sting obtained by radiant heat. Some of the reports follow.

W. "Warmth, particularly at the higher intensities, is much like sting, and a high degree of warmth has the promise of sting. There are times when it is impossible to say when warmth changes to sting. The two qualities are, however, quite different, and the change from the one to the other is quick and smooth."

H. "As warmth got more intensive it became more alive and very weakly stingy. I could find no point where warmth changes to sting;" "there was a change from a rather soft, loose warmth to a more alive, penetrating, slightly stingy quality; I could not tell just where the change takes place; it is almost like a change in intensity;" "warmth itself seems to grow until it gets stingy."

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[4]We realize, in particular, that a radiant source, however carefully controlled, must be physiologically suspect. We hope to return to the problem with another technique.

[5]F. Cutolo, Jr., A Preliminary Study of the Psychology of Heat, this JOURNAL, xxix, 1918, 445ff. J. H. Alston, *op. cit.*, 312, gives some evidence for the view that heat is more like cold than warmth.

B. "At first an increase in warmth; then, as soon as warmth reaches a certain intensity, something else, a prickiness or sting, comes in which is more like pain than pressure; I should say that warmth is like pressure on one side and like sting on the other."

Our cases are too few to permit of a generalization; but we think it probable that further observation will place warmth in a pressure-pain continuum between pressure and sting (heat); no one who is familiar with warmth would hesitate to say that it is like pressure, and our *Os* unite in saying that, under our conditions, warmth is also like sting.

Conclusions. (1) The simultaneous stimulation of warm and pain spots may result in an experience which is variously called 'burning heat,' 'burn', and 'hotness'. Psychologically it seems to be a fusion of the prick quality of pain with either warmth or sting. It is best obtained when the warmth is focal and of good intensity, and the pain not too intensive. At similar intensities this fusion differs from 'actual burn' only in extensity.

(2) The continuous stimulation of a warm spot with radiant heat of constant intensity results in a series of qualities from warmth through sting and burn to pain.[6] The 'sting' obtained in this way is similar to (if not identical with) the quality obtained from the simultaneous stimulation of warm and cold spots, and it was found to have a qualitative likeness to warmth. There is some evidence, therefore, for the statement that warmth lies in a qualitative pressure-pain continuum.

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[6]Warmth, sting (heat), and pain we take to be simple qualities, although we are not prepared to name the quality of the final pain.

BOOK REVIEW

Senescence: The Last Half of Life. By G. Stanley Hall. New York, D. Appletion & Co., 1922, 518 pp.

Having recently retired from the presidency of Clark University, which in 1889 he founded with the financial assistance of Mr. Jonas G. Clark of Worcester, Mass., Dr. Hall decided to take stock of himself, to go over himself physically and mentally most carefully, and to ascertain what it really means to be old (since that is what he was, according to established chronological standards), and how closely it approaches the condition agreed upon for it by long-inherited public opinion. That there was a wide discrepancy between the two will be seen later. Moreover, having spent many years of his life as a genetic psychologist in the study of infancy and childhood, puberty and adolescence, and later of adulthood and sex maturity, he felt that completeness of programme required that he study the last two stages of human life, viz., senescence (from forty, or earlier for women) to the climacteric; and senectitude, which is post-climacteric, or old age proper. Accordingly, he visited half a dozen or more physicians and experts, only to find how little was their knowledge and how great their disagreement concerning this stage of life. The library yielded him some five hundred books and articles on the subject, and the present volume is the result of all these investigations, studies, and the reflections based upon a long life creatively active in many fields (in some of which he was the pioneer) and upon a memory most abundantly and systematically stored with facts gleaned from all the sciences cognate to his own and from history and literature. Thus, while most of the book is in the nature of a comprehensive compendium, it may be said to be almost throughout both original and autobiographical, because not only is the Hall-mark on all the materials that percolated through his mind, but they are transformed, enriched, and given a setting in a larger whole, of which the original authors probably knew next to nothing.

The opening sentence of the Foreword reads: "In this book I have tried to present the subjects of Old Age and Death from as many viewpoints as possible in order to show how the ignorant and the learned, the child, the adult, and the old, savage and civilized man, pagans and Christians, the ancient and the modern world, the representatives of various sciences, and different individuals have viewed these problems, letting each class, so far as I could, speak for itself."

The fundamental thesis of the book is that senescents have a most important function in the world, particularly in these troublous days; one which they have not themselves appreciated or measured up to, because they have not realized "what ripe and normal age really is, means, can, should, and now must do, if our race is ever to achieve its true goal." This function is to distil from the experience of the past the wisdom necessary for the wholesome life of the present, "to gather the fruitage of the past and to penetrate further into the future." And old age is perculiarly fitted to do this because "withdrawal from biological phyletic functions is often marked by an Indian summer of increased clarity and efficiency in intellectual work. Not only does individuation now have its innings but the distractions from passion, the lust for wealth and power, and in general the struggle for place and fame, have abated and in their stead comes normally, not only a philosophic calm but a desire to sum up and evaluate all of life's experiences." Furthermore, the old are disillusioned, they see through the shams and

vanities of life, hence are better guides in the realms of politics, civics, economics, social relationships, sex, marriages, and the family, as well as religion and philosophy. The world sorely needs the disillusionment, the perspective, "the aloofness, impartiality, and power of generalization that age can best supply....These were the qualities that enabled the venerable Joffre to make his masterly two-week's retreat at the Marne. It was done against the will and wish of every one of his younger generals, who now admit he saved Paris and the war and that he was, in a sence, a true superman. The world never so needed the wisdom, which learning cannot give, that sees the vanity and shallowness of narrow partisanship and jingoism, of creeds that conceal more than they reveal, of social shams that often veil corruption, the insanity of the money hunt that monopolizes most of the energy of our civilization, and realizes that with all our vaunted progress man still remains essentially juvenile—much as he was before history began. ....What the world needs is a kind of higher criticism of life and all its institutions to show their latent beneath their patent value by true supermen who, like Zarathustra, are old, very old, with the sapience that long life alone can give. We need prophets with vision who can inspire and also castigate, to convict the world of sin, righteousness, and judgment. Thus there is a new dispensation at the door which graybeards alone can usher in. Otherwise humanity will remain splendid but incomplete. Heir of all the ages, man has not yet come into his full heritage. A traveller, he sets out for a far and supreme goal but is cut off before he attains or even discerns it. The best part of his history is yet unwritten because it is unmade."

"Perhaps in the large Aristotelian sense of the word politics is *par excellence* the work of and for old age....From the patriarchs down the old have been the wisest shepherds of the people, and if young men have succeeded in diplomacy it is because they have been prodigies of precocity who have also devoted themselves to an intensive study of history, which is at best only a proxy for experience....The old who are really so, who are not merely spent projectiles, relics, vestiges, or ruins that time has chanced to spare, do sometimes attain vision and even prophetic power, and their last real words to the world they are leaving are not like the insane babblings of the dying, which friends so often cherish, but are often the best and most worth heeding by their juniors of all their counsels. Some have told us that if the long awaited superman ever arrives, he will come by way of the prolongation of adolescence and others have said it would be by the fuller maturity of man in his prime. No doubt both these stages of life would be enriched and potentialized, but his first advent and his greatest improvement over man of today will be in the form of glorified old age. Nietzsche was right in making Zarathustra old and he himself was the overman whose message he brought to the world. He was intent on the future of man and not on his present, still less on his past."

Dr. Hall fully realizes that there is no virtue in old age as such. Indeed, there are many old who are anything but venerable, wise, or good. Such virtues as old age has are fully earned, not inherent. "Many of those who attain advanced years are battered, water-logged, leaky derelicts without cargo or crew, chart, rudder, sail, or engine, remaining afloat only because they have struck no fatal rocks or because the storms have not quite yet swamped them; or, to change the figure, because they have withered, not ripened on the tree....A psychological senility that neither learns nor forgets is always a menace and a check instead of being, as true old age should be, a guide in emergencies. Thus we have not grown old aright and are paralyzed by a wisdom that is obsolete or barnacled by prejudice, How often it is said of reforms great and good that they are earnestly needed and entirely practical but must wait for their accomplishment until certain venerable but obstructive personages of a generation that is passing are out of the way, because they are prone to think the old good and the new bad, and that every change, therefore, must be for the worse. Thus many live too

long and undo the usefulness of their earlier years....It is because there are so many such that the rôle assigned to the best of us is often so hard and so repugnant to our nature and to our needs....The very little that is known of old age is so predominantly of its inferior specimens, its unfavorable traits and defects and limitations that some old have been prone to repudiate their years, while others are sorely tempted to accept a sham old age that is false to the best that is in us, instead of justifying and illustrating a better one."

"Ripe old age has been a slow, late, precarious, but precious acquisition of the race, perhaps not only its latest but also its highest product. Its modern representatives are pioneers and perhaps its task will prove to be largely didactic. It certainly should go along with the corresponding prolongation of youth and increasing docility in the rising generation if we are right in charging ourselves with the duty of building a new story to the structure of human life....To repeat, our function is to finish a structure that still lacks an upper story and give it an outlook or conning tower from which man can see more clearly the far horizon and take his bearings now and then by the eternal stars."

The above quotations are from the eighth chapter, which is the concluding chapter dealing with old age. The next and last chapter deals with the Psychology of Death. The other chapters may be briefly summarized as follows: Chapter 1. *The Youth of Old Age.* At about 35 or 40 there comes a realization that the tide that 'drew us out the boundless deep' begins to 'turn again home'. This is the dangerous age. Both sexes realize that they face the bankruptcy of some of their youthful hopes, and certain temperaments make a desperate, now-or-never effort to realize their extravagant expectations and are thus led to excesses of many kinds; while others capitulate to fate, lose heart, and perhaps even lose the will-to-live. Quotes Osler, "the evil genius, the croaking Poe raven of this period," whose two fixed ideas were "the comparative uselessness of man above forty years of age and the uselessness of men above sixty years of age, and the incalculable benefit it would be in commercial, political, and in professional life if, as a matter of course, man stopped work at this age." Quotes E. G. Dexter, W. A. Newman Dorland and E. S. P. Haynes as disputing these conclusions, and adds: "If and so far as Osler is right, it is because man up to the present has been abnormally precocious, a trait that he inherited from his shorter-lived precursors and has not yet outgrown, as is the case with sexual precocity, which brings premature old age. Modern man was not meant to do his best work before forty, but is by nature, and is becoming more and more so, an afternoon and evening worker. The coming superman will begin, not end, his real activity with the advent of the fourth decade. Not only with many personal questions but with most of the harder and more complex problems that affect humanity we rarely come to anything like a masterly grip till the shadows begin to slant eastward, and for a season, which varies greatly with individuals, our powers increase as the shadows lengthen. Thus as the world grows intricate and the stage of apprenticeship necessarily lengthens it becomes increasingly necessary to conserve all those higher powers of man that culminate late and it is just these that our civilization, that brings such excessive strains to middle life, now so tends to dwarf, making old age too often *blasé* and *abgelebt*, like the middle age of those roués who in youth have lived too fast." Chapter 2. *History of Old Age.* Treats of the various ages attained by plants and animals, and the attitude toward (and treatment of) the aged among primitives, the ancient Hebrews, Greeks, Romans, the Middle Ages, and children. Everywhere, except among the Hebrews, the lot of the old was pathetic and even tragic, being ignored, neglected, abandoned, shorn of power and authority, put to death, and in times of famine eaten. The views of Cornaro, Bacon, Addison, Robert Burton and Jonathan Swift are quoted. Also Karl Pearson's theory of witchcraft as a revival of the ancient and widespread

matriarchate...."In the eternal struggle of old people to maintain their power against the oncoming generations which would submerge or sweep them away, witchcraft on this view represents the very latest stage of a long and losing struggle of old women for place and influence who in the last resort did not scruple, handicapped though they were by ugliness, neglect, and contempt, to cling to the last remnants of their ancient prerogatives." Chapter 3. *Literature By and On the Aged.* Describes the various attitudes of literary men and women toward old age, followed by quotations from (or résumés of) the works of Margaret E. White, Harriet E. Paine, Amelia E. Barr, Mortimer Collins, Col. Nicholas Smith, Bryon C. Utecht, J. L. Smith, Sanford Bennett, G. E. D. Diamond, Cardinal Gibbons, John Burroughs, Rollo Ogden, James L. Ludlow, Brander Matthews, Ralph Waldo Emerson, Oliver Wendell Holmes, Senator G. F. Hoar, William Dean Howells, H. D. Sedgwick, Walt Mason, E. P. Powell, U. D. Wilson, D. G. Brinton, N. S. Shaler, Anthony Trollope, Stephen Paget, Richard LeGallienne, G. S. Street, C. W. Saleeby, Bernard Shaw. Chapter 4. *Statistics of Old Age and its Care.* Life is probably twice as long as it was three or four centuries ago, and is increasing more rapidly now than ever. The rate of progress is very variable in different countries, the maximum being in Prussia. Improvement is most in females and the rate of increase is accelerated perhaps four years a century on the whole, although during the last three quarters of the nineteenth century Irving Fisher thinks it has increased nine years. At least fourteen years could be added to human life by eliminating preventable diseases. Offsetting the increase in the length of life is the intensity of modern life and industry, which steadily reduces the age of maximal efficiency so that the handicap of years is felt earlier in life than formerly....Not only is the average length of human life increasing as civilization advances, but so is the relative and absolute number of old people, and those who now attain 60, 70, 80 and above are on the average far more comfortable than ever before. However, conditions of life in the modern city, and especially since the Industrial Revolution, are far from being ideal for the old....Nearly every civilized country makes some provision for its aged poor. There are old-age pensions, insurance, annuities, and various provisions made by private corporations, unions, fraternal orders, insurance companies, and philanthropic foundations. The U. S. government is the only one that has no retiring system or provision for old age for its employees, save for soldiers and judges of the Supreme Court....In institutions for the old they suffer most from mass-treatment, for they are not a class but are hyperindividualized, and need most of all personal attention. There is the greatest diversity in food, regimen, and in most bodily and psychic needs....All have their own tastes, aptitudes, habits, as well as mementos and keepsakes, which should always be respected, and every possible facility should be given not only for visits and correspondence but for current reading in order to maintain a larger surface of contact with the world without. The old thus constitute, in a sense, a privileged and even a new "leisure class." Chapter 5. *Medical Views and Treatment of Old Age.* There are no gerontologists as there are experts for women and children, and therefore each senescent must be his own physician. Body-keeping for the old is a very pressing and personal problem requiring much time and attention, and the methods that are successful differ so widely that the diet and regimen good for one might be dangerous, if not fatal, for another....The view so commonly held, that heredity is the chief factor in longevity, is doubtless correct in general. But it is fatalistic and directly tends to lessen the confidence of hygienists and physicians in the efficacy of all their methods of prolonging life in the aged... The psychological effect of this dogma of the prepotence of heredity in determining the length of life is itself not only depressing but may readily become, as psychologists can best understand, a dangerous lethal agent with the old and cause those who have reached the span of years at which their forbears

died to succumb to their troubles with less resistance. Indeed it is one of the chief purposes of this volume to show that the old age problem is not merely economic, philanthropic, social, or even medical, but also, when all is said and done, perhaps chiefly psychological, and that the future welfare of the race depends upon the development of an old age due not chiefly to heredity but to better knowledge and control of the conditions of this state of life. Chapter 6. *The Contributions of Biology and Physiology.* Discusses succinctly the views on the cause of old age of Weismann, Elie Metchnikoff, C. S. Minot, Charles Manning Child, Jacques Loeb, Northrup, Carrel, Pearl, Pozzi, Claude Bernard, and the experiments of Brown-Séquard, Eugene Steinach, Serge Voronoff and their pupils on the rejuvenating effects of testicular fluids taken from young animals and injected into older ones; also of glandular transplantation and grafting; and concludes that "the only practical hope of easement from the hardships of senescence and for the postponement of death now tenable is that now arising faintly and tentatively that, some day, some mitigation of the terrors of old age and death may be found by glandular implantation or perhaps even by the injection of secretions of certain glands....These studies are yet, however, in their infancy and it will be, at the best, a long time before we can know whether they are able to fulfill their promise to the human heart and to the will to live." Chapter 7. *Report on Questionnaire Returns.* Sent the following questionnaire to a few-score eminent and distinguished Americans. How and at what age did you first realize the approach of old age? To what do you ascribe your long life? How do you keep well, that is, what do you find especially good or bad in diet, regimen, interests, and personal hygiene generally? Are you troubled with regrets for things done or not done by or for you? What temptations do you feel, old or new? What duties do you feel that you still owe either to those about you or to the world? Is your interests in public, community, or in far future or past things, as compared with interest in persons and things right about you, greater or less than formerly? In what do you now take your greatest pleasure? Do you enjoy the society of children, of young people, adults, or those near your own age more or less than formerly? Would you live your life over again? Did you experience an "Indian summer" of revived energy before the winter of age began to set in? Do you rely more or less on doctors or find that you must study yourself and be your own doctor? Do you get more or less from the clergy and the church than formerly? Do you think or worry about dying or the hereafter more or less than formerly? Though respondents belonged practically to the same class, yet because of their age their individuation was so great as practically to preclude uniformity in their replies. As many respondents, almost so many different replies. Chapter 8. *Some Conclusions.* "To learn that we are really old is a long, complex and painful experience. Each decade the circle of the Great Fatigue narrows around us, restricting the intensity and the endurance of our activities....At sixty we realize that there is but one more threshold to cross before we find ourselves in the great hall of discard where most lay their burdens down and that what remains yet to do must be done quickly. Hence this is a decade peculiarly prone to overwork. We refuse to compromise with failing powers but drive ourselves all the more because we are on the home stretch. We anticipate leaving but must leave things right and feel we can rest up afterwards. So we are prone to overdraw our account of energy and brave the danger of collapse if our overdraft is not honored. Thus some cross the conventional dead-line of seventy in a state of exhaustion that nature can never entirely make good. Added to all this is the struggle, never so intense for men as in the sixties, to seem younger, to be and remain necessary, and perhaps to circumvent the looming possibilities of displacement by younger men. Thus it is that men often shorten their lives and, what is far more important, impair the quailty of their old age, so that we yet see and know

little of what it could, should, or would be if we could order life according to its true nature and intent Only greater easement between fifty and seventy can bring right, healthful, vigorous senectitude, the services of which to the race constitute probably the greatest of our civilization today....The seventieth is the saddest of all birthdays and if we 'linger superfluous on the stage', we feel that society regards us as, to some extent, a class apart; and so we instinctively make more effort to compensate our clumsiness by spryness and gently resist the kindly offices and tokens of respect to which the young incline or, perhaps more often, are taught to render the old....It is not strange that one of our grievous dangers is patheticism. One who begins to suspect waning love on the part of those in his sphere may come to accept and even crave pity in its place.... There are fathers who, with no thought that they are selfish, monopolise the love and services of their daughters, and mothers who do the same of grown sons....The old often feel a falsetto invalidism....are particularly prone to develop peculiarities which tend to alienate those nearest to them, such as faults in table manners, neglect of toilet, soiled attire, objectionable noises in presence of others, pryingly overcurious, fault-finding, exacting, forgetful....The old are subject to certain fluctuations, new in kind, degree, or both. Sleep is less regular....appetites fluctuate and may readily become capricious....There are alternations of moods....greater dependence upon weather, climate, and seasons, winter being the hardest and spring the easiest....even sex often does not decline and die without terminal oscillations in its course and in extreme cases apathy and aversion may alternate with abnormal erotic outbreaks dangerous alike to the health of the individual, to domestic happiness, and even to public morals.... There is mental starvation because the supplies of mental pabulum fail owing to the reduction of sensations and movements....the days and years pass more slowly and fatigue sets in more quickly....emotivity probably increases with years and most expressions of it, unless they become more sublimated, strongly tend to grow more crass and stormy. We were never more interested in things, persons, events, causes, in life itself. Slights rankle, neglect chills, attentions warm, affronts incense, and praise thrills us, and if we grow censorious, it is because our ideals of conduct and motive have become higher and purer and we are in a greater hurry to see them realized."

"Old age is called second childhood. This is all wrong for there is nothing rejuvenative about it. Childhood is the most active, healthful, buoyant, and intuitive stage of life; age, the least so....The problem of personal hygiene looms up with new dimensions. In our prime we give little attention to health....but now our credit at the bank of health begins to run low....we must select the items of our dietary with discretion and self-restraint....If current events impress and absorb us less, we knit up the past, present, and future into a higher unity." The remainder of this chapter, which is the best and most original in the book, was epitomised in the opening paragraphs of this review. Chapter 9. *The Psychology of Death*. "From infancy to old age the conceptions of death undergo characteristic changes in the individual not unlike those through which the race has passed....Death is not only the king of terrors but to the genetic psychologist every fear is at bottom the fear of death, for all the scores of phobias that prey upon man are of things and of experiences that abate life....The fear of death or of life-abatement for the individual is no whit less pervasive and dominant than are love and hunger, which are so often said to rule the world....Man became man when he knew he must die, and to defer or escape death has been the basal motivation of all his culture.... Man may thus be redefined as the death-shunner. He does not and cannot begin to realize how much he fears death and dreads it now and always has ....The most essential claim of Christianity is to have obviated through its doctrines of the other world, resurrection, and personal immortality,

the fear of death and made the king of terrors into a good friend, if not into a boon companion....but this belief persists only as a dead article of faith which men no longer live by. It is a desiccated herbarium specimen and not a living plant...."

The psychical factors that have overdetermined the hope-wish of personal immortality are as follows:

(1) The desire to be remembered and esteemed by survivors.

(2) The desire to do things that will affect those who survive or will perpetuate our will and works to those who know little or nothing of us or of our name. "Jubal's fame and art filled all the sky, while Jubal lonely laid him down to die," supremely happy in the thought that he had done the race a great service.

(3) The third killer of the death-fear is children and posterity.

(4) The need of another world and life to compensate for the wrongs and imperfections of this one. However, our actual *modus vivendi* is as if another life did not exist and death were the end. No priestcraft can longer make men content with misery here in the hope of compensation hereafter. All make the most and best of this life as if it were all they were sure of, and the motto of most believers is, "One life at a time and this one now."

(5) Another killer of the death-fear is the discovery of physics and chemistry that "death is not only non-existent but inconceivable.... Matter is not only not dead but more intensely active than mind. Transformations take place, but not a single ion dies or is lost."

(6) Mysticism, pantheism, and noetic theories of immortality teach the same thing concerning the individual soul. It does not die but becomes absorbed into the All-Soul.

(7) Philosophy too, from Plato on, has advanced many arguments to prove the immortality of the soul.

"But the fear of death and the forms of mitigating this fear are chiefly because man still dies young. If we had experienced and explored senescence fully we should find that the lust of life is supplanted later by an equally strong counter will to die. We should have no immortality mania for we should be satisfied with life here without demanding a sequel to it. Our present dreams of all forms of post-mortem existence would become a nightmare. True macrobiotism means not only more years but completeness of experience, absence of repression and limitation. Had we lived out the whole of our lives and drained all the draughts of bitter and sweet that nature has ever breathed for us, we should feel sated.

"The fact is, man is now cut off in his prime with many of his possibilities unrealized. Hence he is a pathetic creature doomed to a kind of Herodian slaughter and because he has dimly felt this he has always cried out to the gods and to nature to have mercy. He has imagined answers to the heartrending appeals he shouted into the void; if man dies shall he live again? and on the warrant of fancied answers has supplemented this by another life, which, when psycho-analyzed in all its processess, means only that he has a sense that the human race is unfinished and that the best is yet to come. And so it is. Man's future on this earth is the real, only, and gloriously sufficient fulfillment of this hope. It will be found only in the prolonged and enriched life of posterity here. The man of virtue will realize all desires and live himself completely out so that nothing essentially human will be foreign to his own personal experience.

"Thus the wish for and belief in immorality is at bottom the very best of all possible augurs and pledges that man as he exists today is only the beginning of what he is to be and do. He is only the pigmoid or embryo of his true and fully entelechized self. Thus when he is completed and has finished all that is now only begun in him, heavens, hells, gods, and discarnate ghosts will all fade like dream fabrics or shadows before the rising sun. All doctrines of another life are thus but symbols and tropes in mythic form of the true superman as he will be when he arrives. The great hope so many

have lived and died by will be fulfilled, every jot and tittle of it, not in our own lives but in the perfect man whose heralds we really are without knowing it. Deathbed visions will come true more gloriously than the dying thought. They hunger for more life but the perfect man will die of satiety passing over into aversion and the story will be completed not in a later number but in this."

Of course the long and painstaking studies that produced the two-volumed *Adolescence*, *Educational Problems*, and *Jesus, the Christ* made possible the production of this last volume in considerably less time and with less labor than were expended upon them; and while the specific gravity, so to speak, of the *Senescence* is not as high as that of its predecessors, nor probably was intended to be, there is no mistaking its authorship from first page to last. Though struck off quickly as a minor work, it shows throughout the master's touch, and it can be safely said that there is in no language a work on the subject so comprehensive and discriminating as this one. Moreover, if the function and service of the supersenescents of the future will be to synthetize, "to draw from accumulated experience and knowledge the ultimate and especially the moral lessons of life—in a word, to sum up in a broader view the net results of all we have learned of the *Comédie humaine*", then has this macrobiotic author proved himself a splendid forerunner of the species he has so ably analyzed and described. In the language of the street, *Senescence* is a big book by a Grand Old Man.

JOSIAH MORSE

University of South Carolina

NOTES

An Emendation

I have always been puzzled by the sentence which, in the first edition of the *Physiologische Psychologie*, leads over from the discussion of Consciousness to that of Attention. The first clause is awkwardly worded; the explanation, so far as I can see, does not explain; and the psychology is assuredly not that of the Wundt we know. One may say, of course, that Wundt did not always write impeccable German; that what is a non-sequitur to us may to him, with his associations thick upon him, have been a good argument; and that in 1874 he had not settled down to any very stable psychology. One may say, also, that the whole matter has today only an antiquarian interest. I have said all these things to myself, and have remained unsatisfied. Now, at last, I think I have the key to the passage: I believe that a slightly different sentence stood in Wundt's manuscript, and that the sentence as we read it embodies a correction made while he was going over his paged proofs.

The printed sentence is as follows. "Indem das Bewusstsein in der Synthese der Empfindungen und in der Association der Vorstellungen sich selbst als ein thätiges erfasst, entsteht jene Aeusserung desselben, welche wir *Aufmerksamkeit* nennen." "In the synthesis of sensations and in the association of ideas consciousness apprehends itself as active: hence (*i.e.*, by way of this apprehension of its own activity) arises that manifestation of consciousness which we call *attention*." I believe that what Wundt originally wrote is as follows. "Indem das Bewusstsein in der Synthese der Empfindungen und in der Association der Vorstellungen ein thätiges Erfassen aufweist, entsteht jene Aeusserung desselben, welche wir *Aufmerksamkeit* nennen." I cannot, naturally, be sure of the verb; but my hypothesis requires a short verb meaning 'shows,' 'exhibits,' and *aufweist* does very well. "In the synthesis of sensations and in the association of ideas consciousness displays an active apprehension: hence (*i.e.*, by way of this activity of apprehension) arises that manifestation of consciousness which we call *attention*." This I take to be good German, sound argument, and true Wundtian doctrine. I believe that Wundt changed it, on the printed page, to the sentence that appears in the book. Why should he have made the change?

One must remember the circumstances: that Wundt had been working under pressure of time, that he was very tired, that the whole theory of apperception—at any rate, so far as this book is concerned—was an afterthought. I think, then, that Wundt, when he read his sentence in proof, had an uneasy feeling that it was not adequate to his intended transition. He was introducing a second systematic thread into his psychology; sensory integration was henceforward to be paired with attention. He had just been saying that consciousness is, essentially, synthesis; now he was to break away from synthesis. But was the original sentence sufficient? Might not an unfriendly critic find fault with its logic? "Indem das Bewusstsein in der Synthese der Empfindungen und in der Association der Vorstellungen ein thätiges Erfassen aufweist,"—might not the unfriendly critic finish it by writing: "entstehen diejenigen Aeusserungen desselben, die wir Sinneswahrnehmung und Associationsgesetz nennen,"—something of that sort? But then one would be back again in synthesis, and the step to attention would still remain to be taken.

The imaginary critic would, no doubt, have been wrong,—wrong, and either careless or obtuse. Wundt, however, was very tired, as I have said,

and was also exceedingly anxious to make his point. The conscious activity which underlies attention must be thrown into sharp contrast with the conscious act of synthesis; the introductory clause must differentiate as well as connect. And so I think that Wundt, whose logical acumen had been dulled by the same fatigue that made him oversensitive, sought at the last moment to clinch his argument by raising the activity of attention to a higher level of consciousness. If synthesis occurred on one plane, and apperception on another, there could be no doubt about the twofold basis of his psychology; and consciousness itself, as somehow active on both planes, would keep the psychology unitary. A muddle, —but, under the circumstances, a very natural muddle!

That is my hypothesis: and, if the reader is interested to prove it, I ask him not to do as I have done in this Note, not to confine himself to the structure and logic of the single sentence on p. 717, but rather to consider the whole make-up of Wundt's ch. xviii. If he is still not persuaded, I ask him to turn to the corresponding passage of the second edition.

E. B. T.

Serial Exposition of Wall-Charts

It is often convenient to be able to show a set of charts at the same place on the lecture-room wall,—whether because space is limited, or because all charts must be seen from the same angle, or because the charts in question form a series to be shown in a fixed order. The following device may be worth recording.

The foundation of the whole arrangement is a heavy wooden frame, like a picture frame; it may be of any convenient dimensions. This frame may carry the chart to be exposed last in order. The separate charts of the series are of heavy paper, to the top corners of which are attached rings of thin wire, extending approximately their full diameter (say, 2 cm.) above the edge of the paper. If these charts are of the same width, it is obvious that two supports will hold them all.

The distinctive feature of our arrangement is the substitution of screws rotated by pulleys for the ordinary hooks. The screws pass through the upper strip (2.5 cm. thick) of the wooden frame. Their forward ends (points) are blunt; their backward ends (heads) are fastened to pulleys, which are set flush with the back of the strip. If a spindle is turned down on the head of each screw, the shoulder of the spindle prevents the screw from passing out of the strip at the back, and the pulley at the back prevents the screw from coming forward. The screws are cylindrical, like machine screws; the grooves, however, are relatively deep and broad (about 3 by 3 mm.) and the interspaces are broad and flat (same dimensions). These dimensions are important, because the wire rings must ride smoothly in the grooves, without jumping out, and must be separated along the screw by sufficient space to prevent their interlocking.

On the back of the right-hand side-strip of the frame, low down so as to come within reach of the lecturer, are two pulleys fastened one above the other. A belt passes over the upper pulley to the screw-pulley at the top-right of the frame, and another belt passes over the lower pulley to the screw-pulley at the top-left.

The charts are hung on the frame in the reverse order of that in which they are to be shown, every ring riding in its own groove on the upper side of the screw. To remove a chart, the lecturer pulls the lower belt, rotating the left-hand screw one full turn. Every left-hand ring is carried forward in the groove, until the front ring drops down over the end of the screw. The lecturer catches the dropping chart, and pulls the upper belt, thus releasing the right-hand ring. After a few practice-trials, the charts come away smoothly and noiselessly.

For sudden or total exposures an ordinary roller-shade may be added. The fixtures are set in the front of the top strip of the frame, outside the screws, and the shade is let up and pulled down by a cord in the regular way. After a chart has been shown, the shade is drawn down, and the chart released from its supports under cover of the shade; then the shade is shot up for the next exposure.—

We have found that a light flat spring, resting along the upper surface of the screw, serves to hold the rings securely in place when they are set in the grooves. We have not found it possible to use a single pulley-belt; the slipping of the belt always, in our experience, allows one screw to rotate further than the other; with the result that the discharge of a ring over the end of one screw may happen twice before the other has discharged its first ring. We have tried to simplify the whole arrangement by using one central ring and one central screw; but, with charts of the size that we require, the twofold support is more satisfactory.

H. G. Bishop

Cornell University

The Titchener Commemorative Volume

A limited number of copies of the Titchener Commemorative Volume are left in stock. Since the sales to date have more than paid the costs of the edition, the Committee in charge of publication have decided to offer these remaining copies to psychologists at the reduced price of two dollars, postpaid. The proceeds of their sale, together with the balance already in hand, will be funded, and the interest will presently be used to establish a prize for meritorious work in experimental psychology.

The volume, which consists of 337 pages of the style and size of the pages of The American Journal of Psychology, contains eighteen studies in various departments of psychology, dedicated to Professor Titchener by colleagues and former students on his completion of twenty-five years of service to Cornell University.

Orders may be sent to D. R. Knight, Morrill Hall, Cornell University, Ithaca, N. Y.

M. F. Washburn
W. B. Pillsbury
K. M. Dallenbach

A New Psychological Periodical

We have received the first three parts of a new psychological magazine, *Psychologische Forschung: Zeitschrift für Psychologie und ihre Grenzwissenschaften*, edited by K. Koffka (Giessen), W. Köhler (Berlin), M. Wertheimer (Berlin), K. Goldstein (Frankfurt a. M.) and H. Gruhle (Heidelberg). The contents are as follows.

Bd. i., Heft 1, 2. 1921. W. Köhler. Zur Psychologie des Schimpansen. M. Wertheimer. Untersuchungen zur Lehre von der Gestalt. i. Prinzipielle Bemerkungen. D. Westermann. Tod und Leben bei den Kpelle in Liberia. K. Koffka. Beiträge zur Psychologie der Gestalt. v. Untersuchungen über Bewegungs- und Verschmelzungsphänomene, von P.C ermak und K. Koffka. E. M. von Hornbostel. Ueber optische Inversion. W. Fuchs. Eine Pseudofovea bei Hemianopikern. Referate: E. Rubin, Synsoplevede Figurer; Studier i psykologisk Analyse.

Bd. i., Heft 3, 4. 1922. K. Lewin. Das Problem der Willensmessung und das Grundgesetz der Assoziation, i. A. Pick. Störung der Orientierung am eigenen Körper: Beitrag zur Lehre vom Bewusstsein des eigenen Körpers. G. Marzynski. Sehgrösse und Gesichtsfeld. K. Koffka. Beiträge zur Psychologie der Gestalt. vi. Ueber die Veränderung von Vorstellungen (Gedächtnis und Gestalt), von F. Wulf. J. Borak. Ueber die Empfindlichkeit für Gewichtsunterschiede bei abneh-

mender Reizstärke. W. Köhler. Ueber eine neue Methode zur psychologischen Untersuchung von Menschenaffen. Referate: A. Argelander, Beiträge zur Psychologie der Uebung, i. Uebungsfähigkeit und Anfangsleistung.

Bd. ii., Heft. 1, 2. 1922. E. Lau. Versuche über das stereoskopische Sehen. K. Koffa. Beiträge zur Psychologie der Gestalt. vii. Experimentelle Untersuchungen über das Entstehen und Vergehen von Gestalten, von E. Lindemann. T. W. Danzel. Die psychologische Bedeutung magischer Bräuche. K. Lewin. Das Problem der Willensmessung und das Grundgesetz der Assoziation, ii. E. M. von Hornbostel. Bemerkungen zu einer "Grundfrage der Akustik und Tonpsychologie." Kleine Mitteilungen: Aus dem psychologischen Institut der Universität Giessen; Aus dem psychologischen Seminar der Universität Kiel. Referate: E. Kretschmer, Körperbau und Charakter; J. L. Entres, Zur Klinik und Vererbung der Huntingtonschen Chorea.

Erratum

In the article by H. E. Starr, "The Hydrogen Ion Concentration of the Mixed Saliva considered as an Index of Fatigue and of Emotional Excitation, and applied to a Study of the Metabolic Etiology of Stammering," published in the preceding number of the Journal, the words *anion* and *cation* have by a printer's error been interchanged in two places: (1) last line of p. 397 and first of p. 398, and (2) lines 15, 16 of p. 398. We ask our readers to make the necessary correction.

INDEX OF AUTHORS

(The names of authors of original articles are printed in SMALL CAPS.)

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